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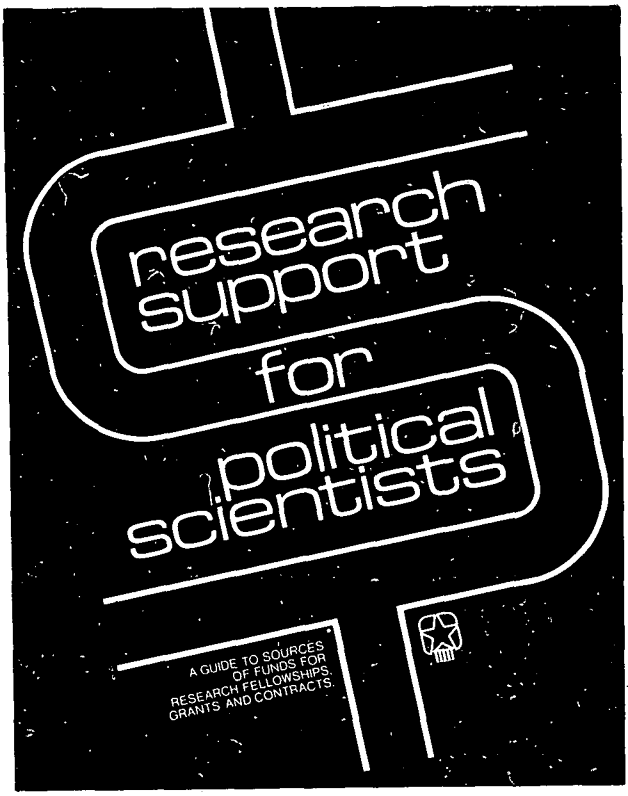
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Approval Voting

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Approval voting is a method of voting in which voters can vote for ("approve of") as many candidates as they wish in an election. This article analyzes properties of this method and compares it with other single-ballot nonranked voting systems. Among the theorems proved is that approval voting is the most sincere and most strategyproof of all such voting systems; in addition, it is the only system that ensures the choice of a Condorcet majority candidate if the preferences of voters are dichotomous. Its probable empirical effects would be to (1) increase voter turnout, (2) increase the likelihood of a majority winner in plurality contests and thereby both obviate the need for runoff elections and reinforce the legitimacy of first-ballot outcomes, and (3) help centrist candidates, without at the same time denying voters the opportunity to express their support for more extremist candidates. The latter effect's institutional impact may be to weaken the two-party system yet preserve middle-of-the-road public policies of which most voters approve.

1. Introduction

In a democratic election between two candidates, election by simple majority is probably the most common voting procedure in use today and has, in addition, certain desirable features (May, 1952; Rae, 1969; Straffin, 1977). When there are three or more candidates, however, and only one is to be elected to a specified office or position, there are widespread differences of opinion as to how the winner should be determined.

These differences are perhaps best epitomized by Condorcet's criticism (1785) of Borda's method (1781), which is recounted along with later proposals in Black (1958). Borda recommended that in an election among three candidates the winner be the candidate with the greatest point total when awards of two points, one point and zero points are made, respectively, to each voter's most-preferred, second most-preferred and least-preferred candidate. Condorcet argued to the contrary that the winner ought to be the candidate who is preferred by a simple majority of voters to each of the other candidates, provided that such a majority candidate exists, and showed that Borda's method can elect a candidate other than the majority candidate. Although a number of writers have accepted Condorcet's criterion, it leaves open the question of which candidate should win when there is no majority candidate (the "paradox of voting"). Fishburn (1977b) reviews a number of methods for determining a winner from voters' preferences when there is no majority candidate and concludes that, although some methods are better than others, there is no obviously best method.

Probably the two most common voting systems used in multicandidate elections today are single and double plurality systems. Under single plurality, each voter can vote for one candidate, and the candidate with the greatest vote total wins the election. Double plurality is a two-ballot system. Its first ballot is the same as the single plurality ballot; the second or runoff ballot is a simple majority ballot involving the two candidates with the largest vote totals from the first ballot. There are of course many other systems that have been or could be used, and we shall mention some of these as we proceed.

A primary purpose of this article is to examine a relatively simple but rarely used type of voting system with several very attractive characteristics. The designation used here for this type of system is *approval voting*. Under approval voting, voters are allowed to vote for ("approve of") as many candidates as they wish but cannot cast more than one vote for each candidate, as under cumulative voting (Brams, 1975, Ch. 3). Voters are not asked to rank their chosen candidates. The winner is the candidate with the greatest vote total.

As in any other system, special provisions must be made for tied outcomes under approval voting, a situation which will be handled here in a general way without specifying an explicit tie-breaking procedure. Although the basic idea of approval voting can be extended to two-ballot systems (Brams and Fishburn, 1978), we shall concentrate on the single-ballot system.

One of the potentially attractive features of approval voting is that it allows voters the maximum

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number of choices in a single-ballot election in which they can vote for, but not rank, the candidates. If there are $m \geq 3$ candidates, under approval voting a voter can cast either an approval vote or no vote for each candidate, giving each voter 2^m possible voting strategies. However, since an abstention (vote for none) has the same net effect as a vote for all candidates, the number of effectively different choices is $2^m - 1$. By contrast, the single plurality system allows $m+1$ different choices (a vote for one of the m candidates or an abstention), and various other single-ballot systems generally allow between $m+1$ and 2^m-1 different strategies if candidates are not ranked by voters.

The effects on voters and potential outcomes of approval voting versus single plurality is vividly illustrated in the 1970 New York State race for United States Senate among James R. Buckley, Charles E. Goodell, and Richard L. Ottinger. In this election, the conservative candidate, Buckley, was elected with 39 percent of the vote under the single plurality system, though either of the more liberal candidates, Goodell or Ottinger, was almost surely preferred by a majority of voters. If approval voting had been used, the outcome probably would have been different since a number of the 61 percent who supported either Goodell or Ottinger (24 percent voted for Goodell, 37 percent for Ottinger) would have voted for both Goodell and Ottinger in an attempt to ensure that Buckley would not win. (In fact, Stratmann [1977] estimated, based on work reported in Stratmann [1974], that under approval voting Goodell would have won with about 59 percent of the vote to about 55 percent each for Buckley and Ottinger; whereas Buckley and Ottinger would have received significant support from Goodell voters, Goodell would have benefited from the support of *both* Buckley and Ottinger supporters.) It may also be noted that Buckley would almost surely have lost under a two-ballot plurality system.

In this article we shall analyze more generally the points suggested by this example. It will be assumed that each voter has a definite preference order on the candidates so that, for any two candidates, a voter prefers one to the other or is indifferent between them (connectivity); and a voter's preference and indifference relations on all candidates are transitive (transitivity). Besides connectivity and transitivity, we will assume that nonabstaining voters use admissible strategies, as defined in section 2, which does *not* imply that they will always vote for their most-preferred candidates. Admissible strategies will also be identified for other types of voting systems and compared to those for approval voting.

In a sense specified precisely in section 3, we shall prove that approval voting is both more sincere and more strategyproof than every other single-ballot voting system that does not ask voters to rank candidates. In other words, among single-ballot systems, approval voting is most likely to encourage voters to report their true preferences, and it is most likely to offer the voter a unique admissible voting strategy.

In section 4 we examine Condorcet's criterion in the special but interesting case of dichotomous preferences. Dichotomous preferences obtain when a voter is able to divide the candidates into a more-preferred subset and a less-preferred subset in such a way as to be indifferent among the candidates within each subset. The main results of the section are that if all voters have dichotomous preferences, then (1) the outcome of a single-ballot election using approval voting will be a Condorcet majority candidate, and (2) approval voting is the only single-ballot nonranked voting system that has this property. If preferences are not dichotomous, the picture is less clear.

In section 5 we consider the possible effects of approval voting on presidential elections. In particular, we focus on the 1968 multicandidate race and report on some estimates, based on survey responses, of the likely outcome had there been approval voting in this election. We discuss probable empirical effects of approval voting and conclude with some normative observations.

2. Admissible Strategies

We begin this section by discussing certain aspects of voters' preferences and then develop a notion of dominance between voting strategies for single-ballot nonranked voting systems. Next we describe the set of all such systems and define the concept of an admissible voting strategy for a given system and a given voter's preference order on the candidates. We then characterize the set of admissible voting strategies for every system and every preference order on the candidates. This is followed by a discussion of admissible strategies for approval voting and a comparison between admissible approval voting strategies and admissible strategies for two other interesting voting systems. Additional comparisons concerning sincere voting strategies and strategyproof voting systems are presented in section 3.

Voters' Preferences. Throughout this article, individual candidates will be denoted as $a, b, c, \cdots$, and subsets of candidates will be denoted as $A, B, C, A_i, \cdots$. For any two subsets A and B, $A \cup B = \{a : a \in A \text{ or } a \in B\}$, the union of A and B, and $A \backslash B = \{a : a \in A \text{ and } a \notin B\}$, the set of all candidates who are in A and not in B. In addition, $\{a\}$ is the subset of candidates that contains only candidate a, $\{a, b\}$ is the subset consisting of candidates a and b, and so forth.

A voter's strict preference relation on the candidates will be denoted as P, so that aPb means that the voter definitely prefers a to b. Similarly, R will denote a voter's nonstrict preference relation on the candidates, so that aRb means that the voter likes a as much as b. Alternatively, aRb means that the voter either strictly prefers a to b or is indifferent between a and b. According to the connectivity and transitivity assumptions noted earlier, the set of all candidates can be partitioned into nonempty subsets, say $A_1, A_2, \cdots, A_n$, for a given P so that the voter is indifferent among all candidates within each A_i and strictly prefers every candidate in A_i to every candidate in A_j if and only if $i<j$. According to this designation, A_1 is the voter's *subset of most preferred candidates* and A_n is the voter's *subset of least preferred candidates*. If the voter is indifferent among all candidates, then A_n and A_1 are the same, but otherwise A_1 and A_n are disjoint. The following comprehensive definition introduces a number of terms that will be used in this and later sections.

DEFINITION 1. Suppose P partitions the set of all candidates into $n \geq 1$ nonempty subsets $A_1, A_2, \cdots, A_n$ so that the voter is indifferent among all candidates within each A_i and has aPb when $a \in A_i$ and $b \in A_j$ if, and only if, $i<j$. Then P is *unconcerned* if and only if $n=1$; P is *dichotomous* if and only if $n=2$; P is *trichotomous* if and only if $n=3$; and P is *multichotomous* if and only if $n \geq 4$. In addition, a subset of candidates B is *high for P* if and only if whenever it contains a candidate in A_j it contains all candidates in A_i for every $i<j$; and B is *low for P* if and only if whenever it contains a candidate in A_i it contains all candidates in A_j for every $j>i$.

A voter who has an unconcerned P will be referred to as an unconcerned voter since s/he is indifferent among all candidates. If P is unconcerned, then every subset of candidates is both high and low for P.

When P is concerned—that is, when P is either dichotomous, trichotomous, or multichotomous—exactly two subsets of candidates are both high and low for P, namely the empty set and the set of all candidates. If P is trichotomous on a set of five candidates $\{a, b, c, d, e\}$, say with $A_1=\{a\}$, $A_2=\{b, c\}$ and $A_3=\{d, e\}$, then P has six high subsets in addition to the two just mentioned, namely $\{a\}, \{a, b\}, \{a, c\}, \{a, b, c\}, \{a, b, c, d\}$ and $\{a, b, c, e\}$; and P has six low subsets in addition to the empty set and the whole set, namely $\{e\}$, $\{d\}$, $\{e, d\}$, $\{c, d, e\}$, $\{b, d, e\}$ and $\{b, c, d, e\}$. In all cases, the number of high subsets is equal to the number of low subsets since a subset is high if and only if its complement (all other candidates) is low.

The characterizations of dominance and admissible strategies to be developed momentarily depend not only on the relations P and R as applied to individual candidates but also on extensions of these relations to subsets of candidates. The reason for this is that we view dominance and admissibility to be based on individual preferences between potential outcomes of a vote, where the *outcome* of a given vote in the single-ballot nonranked context is the set of all candidates who have the greatest vote total from the single ballot. In most real cases there will be no ties and hence the outcome will consist of the one candidate with the largest vote total. However, if ties occur for the largest total, then the outcome will be a subset of two or more candidates.

Instead of specifying a method for determining an ultimate winner when the outcome of a vote contains two or more candidates, we shall proceed on the basis of assumptions that relate preferences between potential vote outcomes to preferences on the individual candidates. The symbols P and R that are used for a voter's preferences between individual candidates will also be used for the voter's preferences between subsets of candidates viewed as potential vote outcomes. Thus, APB means that the voter prefers outcome A to outcome B, and ARB means that s/he finds A as preferable as B.

When A and B are one-candidate subsets, say $A=\{a\}$ and $B=\{b\}$, we shall naturally assume that APB if and only if aPb, and that ARB if and only if aRb. It is assumed also that, for any nonempty A and B, APB and BRA cannot both hold. In addition, the following will be assumed for all candidates a and b and for all subsets of candidates A, B and C:

ASSUMPTION P. If aPb then $\{a\}P\{a, b\}$ and $\{a, b\}P\{b\}$.

ASSUMPTION R. If $A \cup B$ and $B \cup C$ are not empty and if aRb, bRc and aRc for all $a \in A$, $b \in B$ and $c \in C$, then $(A \cup B)R(B \cup C)$.

Assumption P asserts that if candidate a is preferred to candidate b, then outcome $\{a\}$ is preferred to the tied outcome $\{a, b\}$, which in turn is preferred to $\{b\}$. This seems quite reasonable, regardless of how the tie between a and b might be broken when $\{a, b\}$ occurs, if the voter believes that a and b each has a positive probability of being elected when the two are tied after the initial ballot. This will surely be the case if ties are resolved probabilistically (by "coin flips") on the candidates in the outcome, but we expect that it will be true also for most other tie-breaking procedures.

Assumption R says that if everything in A is at least as good as everything in B and C, and if everything in B is at least as good as everything in C, then outcome $A \cup B$ will be as good as outcome $B \cup C$. If ties are broken randomly, then the conclusion of Assumption R says that the random choice of a winner from the union of A and B

is as good as the random choice of a winner from the union of *B* and *C*.

Although Assumption *R* can be expected to hold in other cases, it is possible to imagine situations that challenge its credibility. For example, suppose $\{a, b, c, d\}$ is the set of candidates, and aPb, bPc, and cPd. Suppose further that there are two ballots, and the lowest candidate is eliminated on the first ballot. If there is a tie between *a* and *d* for the lowest vote total, then either $\{a, b, c\}$ or $\{b, c, d\}$ will be in the runoff. Assume that $A \cup B = \{a, b, c\}$ and $B \cup C = \{b, c, d\}$. If the voter with preference order *P* is convinced that *c* will be elected in a runoff among *a*, *b* and *c* and that *b* will be elected in a runoff among *b*, *c* and *d*, then we would expect this voter to prefer $B \cup C$ to $A \cup B$ in contradiction to Assumption *R*. Despite this possibility, Assumption *R* seems plausible in most situations and will be used in our analysis.

We note in passing that Assumption *R* implies that an unconcerned voter will be indifferent among all outcomes as well as among all individual candidates. It then follows from the definition of dominance developed below that an unconcerned voter never has any dominated strategies.

Dominance Between Strategies. In the general context of single-ballot nonranked voting systems, a *strategy* is any subset of candidates. For identification purposes, we shall usually use *S* and *T* rather than $A, B, C, \cdots$ to identify strategies. A voter uses strategy *S* if s/he votes for each candidate in *S* and no candidate not in *S*.

A strategy is *feasible* for a particular voting system if and only if it is permitted by that system. We shall assume that the abstention strategy—which is the empty subset of candidates—is always feasible. For every other strategy a voter's ballot is counted if and only if s/he uses a feasible strategy. In addition to abstention, single plurality has *m* feasible strategies when there are *m* candidates. Under approval voting, all strategies are feasible.

Our central notion of admissible strategies depends on feasibility and on dominance. Roughly speaking, strategy *S* dominates strategy *T* for a particular voter if s/he likes the outcome of *S* as much as the outcome of *T* in every possible circumstance and strictly prefers the outcome of *S* to the outcome of *T* in at least one circumstance. To define dominance precisely, we first define a *contingency* as a function *f* that assigns a nonnegative integer to each candidate. A contingency is interpreted as specifying the numbers of votes each candidate receives from all voters *other than* the voter for whom dominance is being defined. Given a contingency *f* and a strategy *S* for our focal voter, we shall let $F(S, f)$ denote the outcome of the vote. That is, $F(S, f)$ is the subset of candidates who have the greatest vote total under *f* and *S*. For any candidate *a* and strategy *S* let $S(a)=1$ if $a \in S$ with $S(a)=0$ otherwise. Then, with $f(a)$ the integer assigned by contingency *f* to candidate *a*, $a \in F(S, f)$ if and only if $f(a)+S(a) \geq f(b)+S(b)$ for all candidates $b \neq a$. That is, a necessary and sufficient condition for candidate *a* to be contained in the outcome is that s/he receive at least as many votes from *all* voters as does every other candidate.

One of the main tasks of our analysis is to determine strategies for a voter that lead to outcomes s/he most prefers. Although different strategies may be preferred under different contingencies, some strategies are uniformly as good as or better than other strategies regardless of the contingency. That is, one strategy may dominate another strategy.

DEFINITION 2. Given the strict and nonstrict preference relations *P* and *R* for a voter, strategy *S dominates* strategy *T*, or *S* dom *T* for this voter, if and only if $F(S, f)RF(T, f)$ for all possible contingencies *f* and $F(S, f)PF(T, f)$ for at least one contingency.

This definition does not require *S* and *T* to be feasible strategies and it is therefore applicable to all single-ballot nonranked voting systems. Feasibility will enter our analysis explicitly through the definition of admissibility that will follow shortly.

Because of the $F(S, f)PF(T, f)$ requirement at the end of Definition 2, it follows from an earlier remark about Assumption *R* that no strategy is dominated if *P* is unconcerned. The following theorem characterizes dominance between strategies for all concerned *P*. The definitions of high and low subsets are given in Definition 1, and $S \backslash T$ is the set of all candidates that are in *S* and not in *T*.

THEOREM 1 (Dominance). *Suppose* P *is concerned and Assumptions* P *and* R *hold. Then* S *dom* T *for* P *if and only if* $S \neq T$, $S \backslash T$ *is high for* P, $T \backslash S$ *is low for* P, *and neither* $S \backslash T$ *nor* $T \backslash S$ *is the set of all candidates.*

Theorem 1 and other theorems are proved in the Appendix.

Although Theorem 1 is predicated on Assumptions *P* and *R*, the necessary and sufficient conditions for *S* dom *T* do not explicitly use the *P* and *R* relations on the outcomes. That is, *dominance between strategies can be determined completely on the basis of the voter's strict preference relation* P *on the individual candidates.* This greatly simplifies the identification of dominated strategies for a voter.

For example, if the set of candidates is $\{a, b, c\}$, and *P* is trichotomous with *a* preferred to *b* and *b* preferred to *c*, then Theorem 1 says that strategy $\{a\}$, under which the voter votes only for his or her most-preferred candidate, dominates strategies

$\{c\}$, $\{a, c\}$, $\{b, c\}$, $\{a, b, c\}$ and the abstention strategy. Moreover, these are the only strategies that $\{a\}$ dominates.

Theorem 1 also tells us that $\{a\}$ does not dominate $\{b\}$ since $\{b\}\backslash\{a\}=\{b\}$ is not low for P. However, $\{b\}$ is dominated by $\{a, b\}$ according to Theorem 1 since $S \neq T$, $\{a, b\}\backslash\{b\}=\{a\}$ is high for P, $\{b\}\backslash\{a, b\}=\varnothing$ (the empty set, or abstention) is low for P, and neither $\{a\}$ nor $\varnothing$ is the set of all candidates.

Under approval voting, this says that if voters consider voting for their second choice b, then they should also vote for their first choice a since the latter strategy is as good as, and sometimes better than, the strategy of voting for b alone. However, under single plurality, a vote for b alone could be a voter's best strategy since in this case $\{b\}$ is not dominated by any other feasible strategy. As we shall show below (see Definition 4), strategy $\{b\}$ is "admissible" for single plurality voting but "inadmissible" for approval voting.

Admissible Strategies. We shall shortly present a theorem that characterizes all admissible strategies for every concerned P and for all single-ballot voting systems that do not ask voters to rank candidates. This will be followed by comments on admissible strategies for approval voting and for two other simple voting systems.

First, however, we need some definitions. Single-ballot nonranked voting systems will be identified by the numbers of candidates that voters are allowed to vote for if they want their ballots to count.

DEFINITION 3. Suppose there are m candidates. Then a single-ballot nonranked *voting system* is a nonempty subset s of $\{1, 2, \cdots, m-1\}$.

As noted earlier, abstention will be considered feasible for all voting systems. Since a vote for all candidates is tantamount to an abstention so far as the determination of the outcome of a vote is concerned, we have not included m as a possible number in s in Definition 3. This will not present problems with our analysis of admissibility since, when it is allowed, a vote for all m candidates (like an abstention) is dominated by some other feasible strategy whenever P is concerned.

According to Definition 3 and the sentence preceding it, the single plurality system is system $\{1\}$. In other words, under single plurality, a voter is allowed to vote for only one candidate. Under system $\{2\}$, each voter must vote for exactly two candidates if s/he wants his vote to count. System $\{1, 2\}$ allows a voter to vote for either one or two candidates. System $\{1, 2, \cdots, m-1\}$ is the approval voting system. Every system s that omits one or more integers in $\{1, 2, \cdots, m-1\}$ is of course different from the approval voting system. As we shall demonstrate in the next two sections, approval voting is superior to all these other systems in several important respects.

For any subset A of candidates, let $|A|$ denote the *number* of candidates in A. Then strategy S is feasible for system s if and only if either S is the abstention strategy or $|S| \in s$. This characterization of feasibility is consistent with Definition 3 and our previous use of the term. It is now combined with the dominance relation to provide a formal definition of admissibility.

DEFINITION 4. Strategy S is *admissible* for system s and preference order P if and only if S is feasible for s and there is no strategy T that is also feasible for s and has T dom S for P.

As noted in the introduction, our analysis is based on the assumption that nonabstaining voters use admissible strategies. Although a certain amount of formal development has been needed to give a precise definition of admissibility, the intuitive sense of admissibility is easily understood. In effect, Definition 4 says that a voting strategy is admissible for a voter within the context of a specific single-ballot nonranked voting system if and only if (i) that strategy is permitted by the system, and (ii) there is no other permissible strategy—for all possible ways in which other voters may vote—that yields an outcome that is as good for our voter, and is better in at least one instance, as the admissible strategy.

Definition 4 suggests that, because of the feasibility requirement, a strategy feasible for each of two voting systems may be admissible for one system but inadmissible for the other. Indeed, in our earlier example in which $aPbPc$, we noted that strategy $\{b\}$ is admissible for single plurality but inadmissible for approval voting.

Before examining these specific systems in more detail, we state a theorem that characterizes all admissible strategies for every voting system and every concerned preference order. In the statement of the theorem and in later discussion we shall let

$M(P)=A_1$, the subset of most-preferred candidates under P,

$L(P)=A_n$, the subset of least-preferred candidates under P,

where A_1 and A_n are as given in Definition 1.

THEOREM 2 (Admissibility). *Suppose* P *is concerned and Assumptions* P *and* R *hold. Then strategy* S *is admissible for system* s *and preference order* P *if and only if* S *is feasible for* s *and either* C1 *or* C2 *(or both) holds:*

C1: *Every candidate in* M(P) *is in* S, *and it is impossible to divide* S *into two nonempty subsets* S_1 *and* S_2 *such that* S_1 *is feasible for* s *and* S_2 *is low for* P;

C2: *No candidate in* L(P) *is in* S, *and there is no nonempty subset* A *of candidates disjoint from* S *such that* A∪S *is feasible for* s *and* A *is high for* P.

Since the abstention strategy satisfies neither $C1$ nor $C2$, it is never admissible for a concerned voter. Because abstention is inadmissible in our formal sense, a vote for all m candidates must likewise be inadmissible if it is permitted. Thus, though we assume that the abstention and "vote for all" strategies are feasible, we omit them from the formal analysis since they are always inadmissible for a concerned voter.

The criteria of Theorem 2 can be applied to determine all admissible strategies for a given s and concerned P. To illustrate, suppose $m=5$ and $s=\{1, 3\}$, which is a rather unorthodox system that allows a voter to vote for either one or three candidates. Given P defined by $A_1=M(P)=\{a\}$, $A_2=\{b, c\}$, $A_3=\{d\}$ and $A_4=L(P)=\{e\}$, the reader can verify that

strategies $\{a\}$, $\{a, b, c\}$, $\{a, b, d\}$ and $\{a, c, d\}$ are admissible both by criterion $C1$ and by criterion $C2$;
strategies $\{a, b, e\}$ and $\{a, c, e\}$ are admissible by $C1$ only;
strategy $\{b, c, d\}$ is admissible by $C2$ only;

and no other feasible strategy is admissible. For example, strategy $\{b\}$ fails $C1$ since it excludes a, and it fails $C2$ since $A\cup\{b\}$ is feasible for s and $A=\{a, c\}$ is high for P: in other words, feasible $\{a, b, c\}$ dominates feasible $\{b\}$.

Theorem 2 provides general criteria of admissibility for all single-ballot nonranked voting systems. Although these criteria do not have a simple interpretation in the general case, they can be rendered much more perspicuous in particular cases, as we shall next show.

Admissible Strategies for Approval Voting. When Theorem 2 is applied to approval voting, we obtain the following result.

COROLLARY 1. *Strategy* S *is admissible for approval voting and a concerned* P *if, and only if*, S *contains all candidates in* M(P) *and none in* L(P).

Hence concerned voters use one of their admissible strategies under approval voting if and only if they vote for every one of their most-preferred candidates and do not vote for any of their least-preferred candidates. Thus, if $m=4$ and a voter prefers a to b to c to d, then his admissible strategies are $\{a\}$, $\{a, b\}$, $\{a, c\}$ and $\{a, b, c\}$. To illustrate a case in which this voter may consider $\{a, c\}$—consisting of the voter's first and third choices—the best admissible strategy, we first state a result of Brams (1976, 1978a, 1978c) that follows directly from Corollary 1.

COROLLARY 2. *A voter has a unique admissible strategy under approval voting if and only if his or her preference order* P *is dichotomous. This unique strategy is the voter's subset of most-preferred candidates.*

Now consider again a voter with preference order *abcd* (i.e., s/he prefers a to b to c to d) and suppose that all other voters have dichotomous preferences with a indifferent to b and c indifferent to d. Some of the others prefer a and b to c and d, which we write as $(ab)(cd)$—with parentheses denoting indifference subsets—while the rest prefer c and d to a and b, or $(cd)(ab)$. Suppose further that all other voters use their unique admissible strategies, so that $f(a)=f(b)$ and $f(c)=f(d)$ for whatever contingency obtains. Now assume that the voter with preference order *abcd* estimates that the difference between $f(a)$ and $f(c)$ is likely to be more than one vote. Then, assuming that $f(a)>f(c)+1$ and $f(c)>f(a)+1$ are each thought to be fairly likely, $\{a, c\}$ will probably be the best strategy for our voter since $f(a)>f(c)+1$ implies that

$$F(\{a, c\}, f) = F(\{a\}, f) = \{a\} \quad \text{and}$$
$$F(\{a, b\}, f) = F(\{a, b, c\}, f) = \{a, b\},$$

and $f(c)>f(a)+1$ implies that

$$F(\{a, c\}, f) = F(\{a, b, c\}, f) = \{c\} \quad \text{and}$$
$$F(\{a\}, f) = F(\{a, b\}, f) = \{c, d\}.$$

Hence $\{a, c\}$ ensures (1) the election of our voter's most-preferred candidate when $f(a)>f(c)+1$ and (2) the defeat of our voter's least-preferred candidate when $f(c)>f(a)+1$.

Comparisons with Other Systems. Because approval voting offers more feasible strategies than single plurality and the other systems identified in Definition 3, it might appear that it will (i) confuse voters by its large number of options and (ii) be more liable to strategic manipulation than other systems. Not only is (ii) categorically false, as we shall show in section 3, but (i) is not justified either, as we shall next show.

The basis of our arguments is the assumption that concerned voters who do not abstain will entertain only admissible strategies, hence that comparisons between different voting systems should depend only on *admissible* strategies for those systems. Thus the question of the number of options should be based on the number of admissible strategies and not on the number of feasible strategies.

We will compare two other systems with approval voting to illustrate numbers of admissible strategies. The first system is single plurality. The second system is $s=\{1, m-1\}$, in which a voter can vote for either one or all but one candidate. Although the latter system may be unfamiliar, we note that it is equivalent to the negative voting system examined by Brams (1977), which seems first to have been proposed by Boehm (1976). Under negative voting each voter is allowed to cast a vote on one candidate. This vote can be either *for* or *against* the candidate. A *for* vote adds one point to the candidate's score, and an *against* vote subtracts one point from the candidate's score. The outcome of a negative voting system ballot is the subset of candidates with the largest *net vote* total (sum of for and against points), which may be negative. Since a vote against a candidate has the same ultimate effect as a vote for every other candidate, the negative voting system is tantamount to system $\{1, m-1\}$. When $m=3$, this system is equivalent to approval voting, but for $m>3$ negative voting has fewer feasible strategies than approval voting. In what follows we shall refer to system $\{1, m-1\}$ as the negative voting system.

The following corollaries of Theorem 2 identify the admissible strategies of a concerned voter under single plurality and under negative voting. Recall that $L(P)$ is the voter's subset of least-preferred candidates. In Corollary 4, $\bar{a}$ denotes the strategy in which the voter votes for all candidates other than candidate *a* (or casts a vote *against* candidate *a*).

COROLLARY 3. *Strategy* $\{a\}$ *is admissible for single plurality and concerned* P *if and only if* a *is not in* L(P).

COROLLARY 4. *Suppose* $m \geq 3$. *Then strategy* $\{a\}$ *is admissible for negative voting and concerned* P *if and only if the voter strictly prefers* a *to at least two other candidates, and strategy* $\bar{a}$ *is admissible for negative voting and concerned* P *if and only if the voter strictly prefers at least two other candidates to* a.

Corollaries 1, 3 and 4, which provide necessary and sufficient conditions for admissible strategies under three different simple voting systems, can be used to identify and compare sets of admissible strategies for various preference relations on the candidates. For example, given the trichotomous preference order $(ab)cd$ on the set $\{a, b, c, d\}$ of four candidates, the sets of admissible strategies for approval voting, single plurality, and negative voting are respectively $\{\{a, b\}, \{a, b, c\}\}$, $\{\{a\}, \{b\}, \{c\}\}$ and $\{\{a\}, \{b\}, \bar{c}, \bar{d}\}$.

The numbers of admissible strategies for all concerned *P* orders on four candidates are shown in Table 1. It is clear that the relative numbers of admissible strategies for the three systems are very sensitive to the specific form of *P*. Although approval voting may offer more admissible strategies than other systems, as when *P* is $a(bc)d$, it may also offer fewer admissible strategies than the others. Hence it is not generally true that, by comparison to other voting systems, approval voting will overwhelm the voter with a wealth of *viable* options.

Table 1. Numbers of Admissible Voting Strategies for Three Voting Systems with Four Candidates

Concerned Preference Order		**Number of Admissible Strategies for**		
		Approval Voting	**Negative Voting**	**Single Plurality**
dichotomous	$a(bcd)$	1	1	1
	$(abc)d$	1	1	3
	$(ab)(cd)$	1	4	2
trichotomous	$(ab)cd$	2	4	3
	$ab(cd)$	2	4	2
	$a(bc)d$	4	2	3
multichotomous:	$abcd$	4	4	3

3. Sincere Voting and Strategyproofness

We now present a general comparison of approval voting and all other simple voting systems that is based on the following notions of sincere strategies and strategyproofness.

DEFINITION 5. Let *P* be a concerned preference order on the candidates. Then strategy *S* is *sincere* for *P* if and only if *S* is high for *P*; voting system *s* is *sincere* for *P* if and only if all admissible strategies for *s* and *P* are sincere; and voting system *s* is *strategyproof* for *P* if and only if exactly one strategy is admissible for *s* and *P* (in which case this strategy must be sincere).

Sincere strategies are essentially strategies that directly reflect the true preferences of a voter, i.e., that do not report preferences "falsely." For example, if *P* is *abcd*, then $\{a, c\}$ is not sincere since *a* and *c* are not the voter's two most-preferred candidates. Since it is generally felt that a democratic voting system should base the winner of an election on the true preferences of the voters, sincere strategies are of obvious importance to such systems.

They are also important to individual voters because, if a system is sincere, voters will always vote for *all* candidates ranked above the lowest-ranked candidates included in their chosen admissible strategies. (To illustrate when this proposition is not true, if *P* is *abcd*, $\{a, c\}$ is admissible under approval voting but obviously not sincere since this strategy involves voting for candidate *c* without also voting for preferred candidate *b*.) Thus, if a candidate voted for by a sincere voter should win, the sincere voter can rest assured that s/he could not have brought about the election of a preferred candidate by choosing a different admissible strategy.

A voting system that encourages sincere voting, it seems, would probably produce higher voter turnout. By allowing voters to tune their preferences more finely, and by forcing them less often to make insincere choices for strategic reasons, approval voting may well stimulate more voters to express themselves at the polls and enhance their attitudes towards the system.

Using Corollaries 1, 3 and 4, we can easily verify that, for the seven prototype preference orders on four candidates given in Table 1, approval voting is sincere in six cases (only *abcd* is excluded), negative voting is sincere in four cases, and single plurality is sincere in only the first three cases. In fact, it is no accident that approval voting is "more sincere" than both of the other systems used for Table 1: as the following theorem demonstrates, approval voting is the uniquely most sincere system of all the simple voting systems identified in Definition 3.

THEOREM 3. *If* P *is dichotomous then every voting system* s *is sincere for* P. *If* P *is trichotomous then the approval voting system is sincere for* P, *and this is the only system that is sincere for every trichotomous* P. *If* P *is multichotomous then no voting system* s *is sincere for* P.

No system is sincere when P is multichotomous because, for every s and every P with four or more indifference subsets, there is an admissible strategy that is not sincere. When there are relatively few candidates in a race, however, it is reasonable to expect many voters to have dichotomous or trichotomous preference orders. Theorem 3 tells us that when voters do not (or cannot) make finer distinctions, approval voting is the most sincere of all single-ballot nonranked voting systems. Its pertinence to elections today is evident.

Even if a voting system is sincere for P, however, it is not strategyproof for P if it allows more than one admissible strategy. Although manipulation of election outcomes by strategic voting in multicandidate elections is an old topic, only recently has it been shown, through the work of Gibbard (1973, 1977), Satterthwaite (1975), Gärdenfors (1976), Kelly (1977), Blin and Satterthwaite (1977), Barberá (1977a, 1977b), and Pazner (1978), that virtually every type of reasonable election method is subject to the influence of strategic voting; very recently, however, Kalai and Muller (1977), Peleg (1978), and Dutta and Pattanaik (1978) have offered new arguments for the robustness of many voting systems.

Like sincerity, strategyproofness seems a desirable property for a voting system to have. If voters have only one admissible strategy, they will never have an incentive to deviate from it even if they know the result of voting by all the other voters.

Since the demands of strategyproofness are more stringent than those for sincere voting, the circumstances that imply strategyproofness are less likely to obtain than the circumstances that imply sincerity. Nevertheless, as with sincerity, the approval voting system is the uniquely most strategyproof of all systems covered by Definition 3.

THEOREM 4. *If* P *is dichotomous then the approval voting system is strategyproof for* P, *and this is the only system that is strategyproof for every dichotomous* P. *If* P *is trichotomous or multichotomous then no voting system* s *is strategyproof for* P.

The second part of Theorem 4 simply says that if P has at least three indifference subsets, then any system s has at least two strategies that are admissible for s and P; hence, s can be manipulated by a choice of one and not another admissible strategy. The first part of Theorem 4 depends on Corollary 2: if P is dichotomous, then $M(P)$ is the unique admissible strategy for approval voting. In addition, if s is not the approval voting system, then there is a dichotomous P such that there are at least two admissible strategies for s and P.

Theorems 3 and 4 provide very strong support for approval voting evaluated against the important criteria of sincerity and strategyproofness, which are investigated further by Fishburn (1978), and, in a cardinal-utility context, by Merrill (1978). (Other properties that approval voting satisfies are delineated in different axiomatizations of approval voting in Fishburn [1977a, 1977c].) In section 4 we consider the propensity of approval voting and other voting systems to elect a candidate preferred by a majority of voters to every other candidate.

4. Dichotomous Preferences

We now demonstrate that, within the context of single-ballot nonranked voting, approval voting is the uniquely best system with respect to Condorcet's criterion of majority rule when preferences are dichotomous. Many of the results in this section have been noted previously by Brams (1976, 1978a, 1978c).

Throughout this section we shall let V denote a finite list of preference orders on the candidates, with each term in the list representing the preference of a particular voter. Clearly, each allowable preference order may appear more than once in V, or not at all. The *Condorcet candidates* with respect to any given V are the candidates in the set Con(V), where

Con(V) = $\{a$: for each candidate $b \neq a$, at least as many terms in V have a preferred to b as have b preferred to $a\}$.

Thus candidate a is in Con(V) if and only if as many voters prefer a to b as prefer b to a for each b other than a.

As is well known, Con(V) can be empty, which occurs for example if $V = (abc, cab, bca)$, i.e., there is a voters' paradox or cyclical majorities. Condorcet's basic majority rule asserts that candidate a wins the election when Con(V) contains only a and no other candidate. Since it is possible for more than one candidate to be in Con(V), as when the same number of voters prefer a to b as prefer b to a, we extend Condorcet's rule to assert that if Con(V) is not empty, then some candidate in Con(V) wins the election. In particular, this will be the case for a single-ballot voting system if every candidate who is in the outcome from the ballot is in Con(V), provided it is not empty.

It has been shown by Inada (1964) that if all preference orders in V are dichotomous, then Con(V) is not empty. Using the results of the preceding sections, which presume Assumptions P and R, we shall prove much more than this, namely that the use of admissible strategies in approval voting when preferences are dichotomous always yields Con(V) as the outcome. Moreover, we shall show that, for any other single-ballot nonranked voting system s, the use of admissible strategies under dichotomous preferences can give an outcome that contains no candidate in Con(V).

The following formulation will be used to express these results more rigorously.

DEFINITION 6. For any finite list V of preference orders on the candidates, and for any voting system s as identified in Definition 3, let $V(s)$ be the set of all functions that assign an admissible strategy to each of the terms in V. For each function α in $V(s)$, let $F(\alpha)$ be the outcome (the set of candidates with the greatest vote total) when every voter uses the admissible strategy that is assigned to his preference order by α.

As an illustration, assume $V = (abc, abc, c(ab))$, consisting of two voters who prefer a to b to c and one voter who is indifferent between a and b but prefers c to both a and b. If s is the single plurality system, then $V(s)$ contains $(2)(2)(1) = 4$ functions since each of the first two voters has two admissible strategies and the third voter has one admissible strategy according to Corollary 3. The outcomes for the four α functions are $\{a\}$, $\{a, b, c\}$, $\{a, b, c\}$ and $\{b\}$. $F(\alpha) = \{a, b, c\}$, for example, if α assigns strategy $\{a\}$ to the first voter and strategy $\{b\}$ to the second voter.

THEOREM 5. *Suppose all preference orders in* V *are dichotomous and* s *is the approval voting system. Then* F(α) = Con(V) *for every* $\alpha \in$ V(s).

Hence, under approval voting and dichotomous preferences, the use of admissible strategies invariably yields Con(V) as the outcome.

To illustrate the dichotomous preferences situation for single plurality, suppose that the candidate set is $\{a, b, c\}$ and there are $2N + 1$ terms in V, one of which is $a(bc)$ with the other $2N$ terms divided evenly between $b(ac)$ and $(ac)b$. Then Con(V) = $\{a\}$. However, if as many as two of the N voters who have the order $(ac)b$ vote for c, then $F(\alpha) = \{b\}$ and $F(\alpha)$ and Con(V) are therefore disjoint. The following theorem shows that a similar result holds for every system other than the approval voting system.

THEOREM 6. *Suppose* s *is a voting system as described in Definition 3 and* s *is not the approval voting system. Then there exists a* V *consisting entirely of dichotomous preference orders and an* $\alpha \in$ V(s) *such that no candidate in* F(α) *is in* Con(V).

Hence, among all single-ballot nonranked voting systems, there is a uniquely best system by Condorcet's criterion when preferences are dichotomous. Since approval voting is both strategyproof and selects Con(V) when voters use admissible strategies and have dichotomous preferences, it is the best possible voting system in two important respects when preferences are dichotomous.

In contrast to the definitive picture obtained for dichotomous preferences, comparisons among approval voting and other single-ballot systems are much less clear when some voters divide the candidates into more than two indifference subsets. The main work to date on the propensities of different single-ballot systems to elect a Condorcet candidate when Con(V) is not empty has been reported in Fishburn (1974) and Fishburn and Gehrlein (1976; 1977). The conclusions in these studies are based primarily on computer simulations to estimate the probabilities that various voting systems will elect the Condorcet candidate when there is a unique such candidate. Without going into details, we may draw the general conclusion from these studies that when all voters have linear preference orders and vote sincerely, the propensity of the approval voting system to elect the Condorcet candidate is comparable to if not better than the propensities of other single-ballot nonranked systems to elect the Condorcet candidate. In conjunction with the dichotomous preferences results of the preceding section, this strongly suggests that approval voting compares favorably with other single-ballot systems on the basis of Condorcet's rule. Additional theoretical support for this view is provided by Weber (1977a; 1977b) using a probabilistic model.[1]

5. Approval Voting and Presidential Elections

Several desirable properties of approval voting is multicandidate elections have been described in previous sections. As a practicable reform, Kellett and Mott (1977) have made a strong case that approval voting be adopted in presidential primaries, which, at least in the early stages, often involve several candidates running for their party's nomination. When Kellett and Mott asked a sample of 225 Pennsylvania voters to "vote for any candidates whose nomination you can support" in the 1976 presidential primary (eight Democratic candidates and eight Republican candidates were listed on two sample ballots), 72 percent of those voting chose to support two, three, or four candidates.

A case for approval voting in national party conventions can also be made. As in primaries, the main effect would probably be to give comparatively more support to moderates that most delegates find acceptable, comparatively less to extremists who are only acceptable to ideological factions in their party.

If there had been approval voting in the 1972 Democratic convention, it seems at least doubtful that George McGovern would have been his party's nominee. Not only did he not have strong support from his party rank and file (Keech and Matthews, 1976, p. 212), but also he was not accorded any reasonable chance of winning in the general election.

Although most general elections are, for all intents and purposes, two-candidate contests, since 1900 there have been several serious bids by third-party candidates in presidential elections (Mazmanian, 1974).[2] The most notable challenges in the first quarter of the century were in 1912, when Theodore Roosevelt won 27.4 percent of the popular vote, and in 1924, when Robert La Follette won 16.6 percent of the popular vote.

More recently, Harry Truman faced defections from both wings of the Democratic Party in 1948. The Progressive party candidate, Henry Wallace, and the States' Rights party candidate, Strom Thurmond, each captured 2.4 percent of the popular vote. Nevertheless, Truman was able to win 49.6 percent of the popular vote to Republican Thomas Dewey's 45.1 percent.

The most serious challenge by a third-party candidate since World War II was that of George Wallace in the 1968 presidential election. We shall shortly analyze this election in some detail to try to assess the possible effects of approval voting in a presidential election.

Most recently, Eugene McCarthy ran as a third-party candidate in the 1976 presidential election. Playing the spoiler role, McCarthy sought to protest what he saw to be the outmoded procedures and policies of the Democratic party, for whose nomination he had run in 1968 and 1972. Although McCarthy garnered only 0.9 percent of the popular vote, his candidacy may have cost Jimmy Carter four states, which Gerald Ford won by less than what McCarthy polled. In the end, of course, Carter did not need the electoral votes of these states, but had he lost in a few states that he won by slim margins, these McCarthy votes could have made the difference.

We turn now to an analysis of the third-party challenge by George Wallace's American Independence party in 1968. As with Strom Thurmond's support 20 years earlier, Wallace's support was concentrated in the South. Although Wallace had no reasonable chance of winning the presidency, it seems at the time that he had a very good chance of preventing both Richard Nixon and Hubert Humphrey from winning a majority of electoral votes, thereby throwing the election into the House of Representatives. There Wallace could have bargained with these candidates for major policy concessions—in particular, weaker enforcement of civil rights statutes and a halt to busing.

Wallace captured 13.5 percent of the popular vote and was the victor in five states, winning 46 electoral votes. He came close to denying Nixon, who got 43.4 percent of the popular vote to Humphrey's 42.7 percent, an electoral-vote majority.

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[1] Recently we (Brams and Fishburn, 1978) have been able to make these arguments more conclusive by proving that (i) among all single-ballot nonranked voting systems, approval voting is the only system that guarantees the existence of sincere admissible strategies that elect a Condorcet candidate (if one exists); (ii) allowing for a runoff election between the top two candidates under plurality voting also ensures the existence of admissible strategies that elect a Condorcet candidate, but they are not necessarily sincere; (iii) if plurality voting (with or without a runoff) leads to the election of a Condorcet candidate whatever admissible strategies voters choose, so does approval voting, but the reverse is not true: there are situations in which approval voting guarantees the election of a Condorcet candidate but plurality voting (with or without a runoff) does not. The last statement says, in effect, that approval voting does a better job of *ensuring* the election of a Condorcet candidate than its main competitors, single-ballot and two-ballot plurality voting; we consider this result on a par with our sincerity and strategyproofness results in that it establishes the "dominance" of approval voting—but in this case with respect to the Condorcet criterion.

[2] An interesting model to explain why "sophisticated" or "disillusioned" voters support third-party candidates under single plurality systems is given in Riker (1976); an alternative model positing "sincere" voters is given in Brams (1978b).

Would this outcome have been different, or would its magnitude have significantly changed, if there had been approval voting in 1968? Presumably, all voters who voted for one of the three candidates would not have changed their votes. But how many would have cast second approval votes, and for whom would they have voted?

The best information available to answer this question was collected in the University of Michigan Survey Research Center's 1968 National Election Study. Data derived from a "feeling thermometer" assessment of candidates—whereby respondents are asked to indicate warm or cold feelings toward the candidates on a 100-degree scale—may be used to define an "acceptability" scale for candidates, from which plausible approval voting strategies of voters can be surmised.

Taking account of both the reported votes of the respondents (the survey was taken just after the election) and their feeling-thermometer assessments for the candidates, Kiewiet (1977) developed a set of rules for assigning approval votes to respondents.[3] After adjusting reported voting by the sample to reflect the actual voting results, Kiewiet estimated that Nixon would have increased his vote total to 69.8 percent (a 58 percent increase over the 44.1 percent in the survey who reported voting for Nixon), Humphrey would have increased his vote total to 60.8 percent (a 44 percent increase over the 42.3 percent in the survey who reported voting for Humphrey), and Wallace would have increased his vote total to 21.3 percent (a 58 percent increase over the 13.5 percent in the survey who reported voting for Wallace.)

Kiewiet draws several conclusions from his analysis. First, single plurality voting nearly deprived Nixon of his victory: although many voters were certainly not wildly enthusiastic about Nixon, more than a two-thirds majority probably considered him at least acceptable. Second, although most of the additional approval votes Nixon and Humphrey would have received would have come from each other's supporters, Wallace supporters—according to the rules used for assigning approval votes—would have cast more than twice as many approval votes for Nixon as for Humphrey.

It is this factor which largely explains Nixon's 9 percent approval-voting edge over Humphrey. Wallace also would have benefited from approval voting. In fact, his estimated 21-percent approval voting share exactly matches the percentage who reported they would vote for him two months before the election (Scammon and Wattenberg, 1970, pp. 170–71). If there had been approval voting, Wallace almost surely would not have lost most of his original supporters, and probably would have picked up some support from the major-party voters as well, to capture approval votes from more than one-fifth of the electorate.

Perhaps the most interesting conclusion we can derive from these estimates is that Nixon was undoubtedly the Condorcet winner. Kiewiet estimates that he would have defeated Wallace in a pairwise contest 81.5 percent to 18.5 percent and would have defeated Humphrey 53.4 percent to 46.6 percent, given the propensity of Wallace voters to favor Nixon.

Several objections can be raised against Kiewiet's estimates and indeed against virtually any estimates based on assumptions about how the attitudes or "feelings" of voters would translate into voting behavior. Rather than dwelling on these, however, let us consider a rather different set of estimates made by Kiewiet based on more "strategic" assumptions.

These assumptions reflect the view of most voters in 1968 that only Humphrey and Nixon stood a serious chance of winning the election. After all, even at his high point in the polls, Wallace commanded the support of barely more than one-fifth of the electorate. It is plausible to assume, therefore, that voters would cast approval votes to distinguish between Humphrey and Nixon (Brams, 1978a, 1978b).

More specifically, Kiewiet assumed that (i) Humphrey and Nixon supporters would vote for Wallace, if they also approved of him, but would not vote for the other major-party candidate; (ii) all Wallace voters would vote for either Humphrey or Nixon, but not both, in addition to Wallace. As he put it,

> In effect, a poll indicating Wallace had no chance of winning would, under approval voting, turn the election into two elections: the first, a pairwise contest between Nixon and Humphrey, wherein all voters would choose one or the other; the second, a sort of referendum for Wallace, who would receive approval votes from voters who wished to support him even if he could not win the election.

In operational terms, Kiewiet postulated that Humphrey and Nixon supporters would vote for their first choice and, in addition, for Wallace if the latter's thermometer rating exceeded 50. Wallace supporters, on the other hand, were assumed always to cast a second approval vote for the major-party candidate they gave the highest thermometer rating to, no matter what this rating was. Thereby Wallace voters were "forced" to be rational in accordance with the assumptions of the poll model.

What estimates does this set of assumptions yield? Nixon would have received 53.4 percent of the popular vote and Humphrey 46.6 percent—the same percentages given earlier had they been in a pairwise contest—and Wallace 21.3 percent. Thus, the approval voting percentages of Humphrey and Nixon would have been substantially reduced over those estimated earlier (69.8 and 60.8 percent, respectively), but Wallace would have come out exactly the same (21.3 percent estimated earlier) since the "strategic" assumptions do not alter the voting behavior of Wallace supporters for Wallace.

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[3] For a related effort, applied to primaries, to translate 1972 feeling-thermometer data into electoral outcomes under a variety of decision rules, see Joslyn (1976).

The two sets of estimates for Humphrey and Nixon probably bracket the percentages the candidates would actually have received had there been approval voting in 1968. Whichever set gives the better estimate, Nixon in either case would have been the clear-cut winner in the popular-vote contest because of the much broader support he, rather than Humphrey, would have received from Wallace supporters.

The Electoral College also magnified Nixon's narrow popular-vote victory because he won by slim margins in several large states. However, speaking normatively, we believe this fact should have no bearing on the outcome. Much more significant is the fact that Nixon was the first or second choice of most voters and hence more acceptable than any other candidate. This, we believe, is the proper criterion for the selection of a president—and other democratically elected officials as well.

It is also interesting to note that approval voting would probably obviate the need for a runoff election in most multicandidate presidential elections if the Electoral College were abolished. No winning candidate in a presidential election has ever received less than 40 percent of the popular vote, with the exception of Abraham Lincoln in 1860, who got 39.8 percent. It seems highly unlikely that a candidate who is the first choice of 40 percent of the electorate would not be approved of by as many as one-sixth of the remaining voters and thereby receive at least 50 percent support from the electorate.

The legitimacy of election outcomes in the eyes of voters would certainly be enhanced if the winning candidate received the support of a majority of the electorate. This would be true even if the winner were the first choice of fewer voters than some other candidate, because this fact would not show up in the approval-voting returns.

By comparison, the proposed popular-vote amendment to abolish the Electoral College provides for a runoff between the top two vote-getters if neither receives at least 40 percent of the vote. This seems an unnecessary provision if more than 50 percent approve of the winning candidate. Of course, if no candidate wins even a majority of approval votes, then a runoff can still be conducted to ensure a majority winner.

But this would probably not be necessary in most presidential elections unless approval voting itself produces major changes in candidate strategies and election outcomes. Beyond these changes, however, approval voting could effect a fundamental alteration in the two-party system itself by encouraging additional parties or candidates to enter the fray. Fringe candidates, it seems, would probably drain little support from centrist candidates because, for strategic reasons, fringe candidate supporters would probably also tend to vote for a centrist. Additional centrist candidates, on the other hand, might draw support away from major-party candidates if they (the new centrists) were perceived as serious contenders.

The question that is hard to answer, in the absence of experience, is whether such contenders could position themselves in such a way as to displace the major-party candidates. If so, presumably they would be motivated to run, giving voters more viable alternatives from which to choose and, in the process, weakening the two-party system. Their election, however, would probably not produce drastic changes in public policy since they would not be viable if they were unacceptable to numerous middle-of-the-road voters.

Barring unforeseen changes, it seems likely that at the same time approval voting would give some additional support to strong minority candidates like George Wallace, it would also help centrist candidates—including perhaps nominees of new parties—both in winning their party's nomination in the primaries and conventions and prevailing against more extreme candidates in the general election. Coupled with the greater opportunity it affords voters to express their preferences, and the greater likelihood it provides the winning candidate of obtaining majority support, approval voting would seem to be an overlooked reform that now deserves to be taken seriously.

In a way, approval voting is a compromise between plurality voting and more complicated schemes like the Borda count which require voters to rank candidates. In our view, the latter schemes are both too complicated and unnecessary in elections in which there is only a single winner. (Elections in which there are multiple winners, such as to a committee or council, would also seem well suited for approval voting, but that is a subject for another article). On the other hand, approval voting is not only quite easy to understand—even if some of its theoretical implications are not so obvious—but it also seems an eminently practicable scheme that could readily be implemented on existing voting machines.

Appendix

Proofs of the technical results of the paper are given in this appendix. It will be assumed throughout that P is concerned and that Assumptions P and R hold. The following notation will be used in addition to the notation introduced in the paper. The empty set is denoted by $\varnothing$; $A\cap B=\{a: a\in A$ and $a\in B\}$, the intersection of A and B; $A\subseteq B$ means that A is a subset of B; $A\nsubseteq B$ means that A is not a subset of B (something in A is not in B); $A\subset B$ means that A is a proper subset of B($A\subseteq B$ and something in B is not in A); and $|A|$ is the number of candidates in A.

THEOREM 1. S *dom* T *for* P *if and only if* $S\neq T$, $S\setminus T$ *is high for* P, $T\setminus S$ *is low for* P, *and neither* $S\setminus T$ *nor* $T\setminus S$ *is the set of all candidates.*

Proof. Suppose first that $S\neq T$ and that the other conditions on S and T stated thereafter in the theorem are true. Then there must be a and b such that aPb and either (i) $a\in S\setminus T$ and $b\notin S\setminus T$, or (ii) $a\notin T\setminus S$ and $b\in T\setminus S$. For example, if $S\setminus T\neq\varnothing$ let a be a most preferred candidate in $S\setminus T$ so that $a\in M(P)$ since $S\setminus T$ is high. If it were then true that aPb never held for $b\notin S\setminus T$, then aPb implies $b\in S\setminus T$ and, since $S\setminus T$ is high, this would require $S\setminus T$ to be the entire set of candidates, which contradicts one of our hypotheses. Therefore, when $S\setminus T\neq\varnothing$, aPb for some $a\in S\setminus T$ and $b\notin S\setminus T$. Similarly, if $T\setminus S\neq\varnothing$ then aPb for some $b\in T\setminus S$ and $a\notin T\setminus S$. Since $S\neq T$ implies either $S\setminus T\neq\varnothing$ or $T\setminus S\neq\varnothing$, either (i) or (ii) must be true.

Let f be a contingency for which $f(a)=f(b)$ with $f(a)>f(c)+1$ for all other candidates c when (i) or (ii) holds. Then

$F(S,f)=\{a\}$ and $F(T,f)=\{b\}$
if $a\in S\setminus T$ and $b\in T\setminus S$;
$F(S,f)=\{a,b\}$ and $F(T,f)=\{b\}$
if either $a\in S\setminus T$ and $b\in S\cap T$ or $b\in T\setminus S$ and $a\notin S\cup T$;
$F(S,f)=\{a\}$ and $F(T,f)=\{a,b\}$
if either $a\in S\setminus T$ and $b\notin S\cup T$ or $b\in T\setminus S$ and $a\in S\cap T$.

Since the enumerated possibilities for a and b cover all possibilities under (i) and (ii), Assumption P and aPb imply $F(S,f)PF(T,f)$. Hence there is a contingency under which the outcome for S is strictly preferred to the outcome for T.

Continuing with the assumptions that $S\neq T$, $S\setminus T$ is high for P, and $T\setminus S$ is low for P, we show next that $F(S,f)RF(T,f)$ for all contingencies f. Under the given assumptions,

$$a\notin T\setminus S \text{ and } b\in T\setminus S \text{ imply } aRb, \tag{1}$$

$$a\in S\setminus T \text{ and } b\notin S\setminus T \text{ imply } aRb. \tag{2}$$

We consider three exhaustive possibilities for a contingency f.

Case 1: $F(T,f)\cap(S\setminus T)\neq\varnothing$. It then follows that $F(S,f)=F(T,f)\cap(S\setminus T)$. To apply Assumption R, let $A=\varnothing$, $B=F(S,f)$ and $C=F(T,f)\setminus F(S,f)$. If $C=\varnothing$, then $F(S,f)=F(T,f)=B$, and BRB by Assumption R. If $C\neq\varnothing$, then each $b\in B$ is in $S\setminus T$ and each $c\in C$ is not in $S\setminus T$ so that bRc by (2); therefore $BR(B\cup C)$, or $F(S,f)RF(T,f)$, by Assumption R.

Case 2: $F(S,f)\cap(T\setminus S)\neq\varnothing$. Then $F(T,f)=F(S,f)\cap(T\setminus S)$. In this case, let $A=F(S,f)\setminus F(T,f)$, $B=F(T,f)$ and $C=\varnothing$ for direct application of Assumption R. If $A=\varnothing$, then $F(S,f)=F(T,f)=B$ with BRB. And if $A\neq\varnothing$, then each $a\in A$ is not in $T\setminus S$ and each $b\in B$ is in $T\setminus S$ so that aRb by (1); therefore $(A\cup B)RB$, or $F(S,f)RF(T,f)$, by Assumption R.

Case 3: Both $F(T,f)\cap(S\setminus T)$ and $F(S,f)\cap(T\setminus S)$ are empty. Hence if $a\in F(T,f)$ then $a\notin S\setminus T$, and if $a\in F(S,f)$ then $a\notin T\setminus S$. Suppose $a\in F(T,f)$ and $a\notin T\setminus S$. Then a is in both T and S or else in neither T nor S, and in each case it follows that $a\in F(S,f)$. Similarly, if $a\in F(S,f)$ and $a\notin S\setminus T$, then $a\in F(T,f)$. Therefore, the set of all candidates in $F(T,f)$ but in neither $S\setminus T$ nor $T\setminus S$ is identical to the set of all candidates in $F(S,f)$ but in neither $S\setminus T$ nor $T\setminus S$. Let B be this common set, and let $A=F(S,f)\cap(S\setminus T)$ and $C=F(T,f)\cap(T\setminus S)$ so that $F(S,f)=A\cup B$ and $F(T,f)=B\cup C$. Since (1) and (2) imply that aRb, bRc and aRc for all $a\in A$, $b\in B$ and $c\in C$, it follows from Assumption R that $F(S,f)RF(T,f)$.

Thus far in this proof we have shown that if the latter conditions on S and T in Theorem 1 are true then S dom T. We now establish the necessity of these conditions for dominance. If either $S\setminus T$ or $T\setminus S$ is the set of all candidates, then one of S and T must be the set of all candidates and the other must be empty, in which case $F(S,f)=F(T,f)$ for all f so that neither strategy can dominate the other. A similar conclusion holds if $S=T$. If either $S\setminus T$ is not high for P or $T\setminus S$ is not low for P then there are a and b such that aPb and either (iii) $a\in T\setminus S$ and $b\notin T\setminus S$ or (iv) $a\notin S\setminus T$ and $b\in S\setminus T$. Then, by the first two paragraphs of this proof (interchange S and T), there is an f such that $F(T,f)PF(S,f)$, and therefore S cannot dominate T.

THEOREM 2. *Strategy* S *is admissible for* s *and* P *if and only if* S *is feasible for* s *and either:*

C1: $M(P)\subseteq S$ *and* S *does not include a nonempty proper subset* B *that is low for* P *and has* $S\setminus B$ *feasible for* s; *or*

C2: $L(P)\cap S=\varnothing$ *and there is no nonempty* A *that is high for* P, *has* $A\cap S=\varnothing$ *and for which* $A\cup S$ *is feasible for* s.

Proof. Given that S is feasible for s we are to show that it is admissible for s and P if and only if C1 or C2 holds. Suppose first that $M(P)\nsubseteq S$ and $L(P)\cap S\neq\varnothing$, and take $a\in S\setminus M(P)$ and $b\in L(P)\cap S$. Form T from S by deleting b and adding a so that $|T|=|S|$ with T feasible. Then $T\setminus S=\{a\}$, which is high, and $S\setminus T=\{b\}$, which is low. Hence, by Theorem 1, T dom S and S is not admissible. Therefore, if S is admissible, it must be true that either $M(P)\subseteq S$ or $L(P)\cap S=\varnothing$.

Suppose next that $M(P)\subseteq S$. If S includes a nonempty proper subset B that is low for P and has $S\setminus B$ feasible for s, then, by Theorem 1, $(S\setminus B)$ dom S since $(S\setminus B)\setminus S=\varnothing$, which is high, and $S\setminus(S\setminus B)=B$, which is low, and therefore S is not admissible. On the other hand, suppose T is feasible and T dom S. Then $T\neq S$ and $T\setminus S$ high require $T\setminus S=\varnothing$ since $M(P)\subseteq S$. Therefore $T\subset S$. If $T=\varnothing$ then $S\setminus T$, which equals S, cannot be low since S cannot be the entire set of candidates (by Theorem 1) and since $M(P)\subseteq S$. Therefore T dom S requires $\varnothing\subset T\subset S$. Let B in the statement of $C1$ in Theorem 2 equal $S\setminus T$. Then B is a nonempty proper subset of S, B is low for P (by Theorem 1 and T dom S), and $S\setminus B=T$ is feasible for s. Therefore, given S feasible and $M(P)\subseteq S$, S is not admissible if, and only if, the latter conditions in $C1$ are false.

Finally, suppose that $L(P)\cap S=\varnothing$. If there is a nonempty A that is high, disjoint from S, and for which $A\cup S$ is feasible for s, then $(A\cup S)$ dom S by Theorem 1 and therefore S is not admissible. (The possibility of $S=\varnothing$ and A being the set of all candidates is ruled out by the exclusion of m from s.) Conversely, suppose T is feasible and T dom S. Then, since $S\setminus T$ is low, $T\neq S$, and $L(P)\cap S=\varnothing$, we must have $S\setminus T=\varnothing$ and therefore $S\subset T$. Let A in the statement of $C2$ equal $T\setminus S$. Then A is nonempty, A is high for P (since $T\setminus S$ is high by Theorem 1), $A\cap S=\varnothing$, and $A\cup S=T$ is feasible for s. Therefore, given S feasible and $L(P)\cap S=\varnothing$, S is not admissible if, and only if, the latter conditions in $C2$ are false.

COROLLARY 1. *Strategy* S *is admissible for approval voting and* P *if, and only if,* $\mathrm{M(P)}\subseteq\mathrm{S}$ *and* $\mathrm{L(P)}\cap\mathrm{S}=\varnothing$.

Proof. Suppose first that $M(P)\nsubseteq S$. Take $a\in S\setminus M(P)$. Then $(S\cup\{a\})$ dom S by Theorem 1 so that S is not admissible (for approval voting and P). Suppose next that $L(P)\cap S\neq\varnothing$. Take $b\in L(P)\cap S$. Then $(S\setminus\{b\})$ dom S by Theorem 1 so that S is not admissible. Admissibility therefore requires both $M(P)\subseteq S$ and $L(P)\cap S=\varnothing$. Assume then that $M(P)\subseteq S$ and $L(P)\cap S=\varnothing$. Since $L(P)\cap S=\varnothing$ implies that there is no nonempty proper subset B of S that is low for P, and $M(P)\subseteq S$ implies that there is no nonempty A that is high for P and disjoint from S, it follows from Theorem 2 that S is admissible.

COROLLARY 2. *There is a unique admissible strategy for approval voting and* P *if and only if* P *is dichotomous, and this unique strategy is* M(P).

Proof. The proof follows immediately from Corollary 1.

COROLLARY 3. *Strategy* $\{\mathrm{a}\}$ *is admissible for* $s=\{1\}$ *and* P *if and only if* $\mathrm{a}\notin\mathrm{L(P)}$.

Proof. By Theorem 2, the only way $\{a\}$ cannot be admissible for single plurality and P is when $a\in L(P)$. Then both $C1$ and $C2$ fail. If $a\notin L(P)$ then $C2$ is true.

COROLLARY 4. *Suppose* $\mathrm{m}\geq 3$. *Then* $\{a\}$ *is admissible for* $\{1, \mathrm{m}-1\}$ *and* P *if and only if* a *is strictly preferred to at least two other candidates, and* $\bar{\mathrm{a}}$ *is admissible for* $\{1, \mathrm{m}-1\}$ *and* P *if and only if at least two other candidates are strictly preferred to* a.

Proof. Let $s=\{1, m-1\}$ with $m\geq 3$. First, if a is strictly preferred to two or more candidates then $L(P)\cap\{a\}=\varnothing$ and if nonempty A is high for P and $a\notin A$ then $1<|A\cup\{a\}|\leq m-2$ so that $A\cup\{a\}$ is not feasible for s. Hence, by $C2$ of Theorem 2, $\{a\}$ is admissible. Second, if at least two candidates are preferred to a then $M(P)\subseteq\bar{a}$ and if B is a nonempty proper subset of $\bar{a}$ that is low then B does not contain any candidate preferred to a so that $2\leq|S\setminus B|<m-1$, in which case $S\setminus B$ is not feasible for s. Hence, by $C1$ of Theorem 2, $\bar{a}$ is admissible. Third, suppose that a is preferred to at most one other candidate. If a is preferred to no other candidate then $L(P)\cap\{a\}\neq\varnothing$ and $C1$ and $C2$ fail; if a is preferred to exactly one candidate then $M(P)\nsubseteq\{a\}$, so $C1$ fails, and, even though $L(P)\cap\{a\}=\varnothing$, $C2$ is seen to fail when A equals all candidates except a and the one in $L(P)$. Hence $\{a\}$ is not admissible in this case. Finally, suppose that at most one other candidate is preferred to a. If nothing else is preferred to a, then $M(P)\nsubseteq\bar{a}$ and $C1$ and $C2$ fail; if exactly one candidate is preferred to a, then $L(P)\cap\bar{a}\neq\varnothing$, so $C2$ fails and, although $M(P)$ may be included in $\bar{a}$, if in fact $M(P)\subseteq\bar{a}$ then with B all candidates other than a and the one in $M(P)$ we see that B is a nonempty proper subset of $\bar{a}$ that is low for P and for which $\bar{a}\setminus B=M(P)$ is feasible, thus implying that $C1$ fails. Hence $\bar{a}$ is not admissible in this final case.

THEOREM 3. *If* P *is dichotomous then every* s *is sincere for* P. *System* s *is sincere for every trichotomous* P *if and only if* s *is the approval voting system. If* P *is multichotomous then no* s *is sincere for* P.

Proof. If P is dichotomous then every S that has $M(P)\subseteq S$ or $L(P)\cap S=\varnothing$ is sincere. Hence, by Theorem 2, every s is sincere for P. If P is trichotomous and s is the approval voting system then it follows immediately from Corollary 1 that s is sincere for P.

We show next that the trichotomous result for systems other than approval voting holds for each $m\geq 3$. For a generic trichotomous P with indifference subsets A_1, A_2 and A_3 as in Definition 1, let $m_i = |A_i|$ with $m_1 + m_2 + m_3 = m$. If s is not the approval voting system then either there is an $i \in s$ with $1 \leq i < m-1$ and $i+1 \notin s$, or there is an $i \in s$ with $1 < i \leq m-1$ and $i-1 \notin s$. Suppose first that $i \in s$, $1 \leq i < m-1$ and $i+1 \notin s$, and let P be such that $m_1 = i$, $m_2 = m-i-1$ and $m_3 = 1$. Form strategy S with $i-1$ candidates from A_1 and 1 candidate from A_2. Then S is not sincere, but it is admissible by C2 of Theorem 2. Suppose next that $i \in s$, $1 < i \leq m-1$ and $i-1 \notin s$, and let P be such that $m_1 = i-1$, $m_2 = m-i$ and $m_3 = 1$. Form S with $i-1$ candidates from A_1 and 1 candidate from A_3: i.e., $S = A_1 \cup A_3$. Then S is not sincere, but it is admissible by $C1$ of Theorem 2.

For the multichotomous case let P have $n \geq 4$ indifference subsets $A_1, A_2, \cdots, A_n$ as in Definition 1, with $m_i = |A_i|$ and $m_1 + m_2 + \cdots + m_n = m$. In addition, let $s = \{s_1, s_2, \cdots, s_r\}$ with $1 \leq s_1 < s_2 < \cdots < s_r \leq m-1$ and $r \geq 1$ be a generic voting system for m-candidate elections. We are to show that s is not sincere for P. If $s_1 > m_1$ let S with $|S| = s_1$ include A_1, exclude something in A_2, and contain something in A_3. Then S is admissible by $C1$ but it is not sincere. Assume henceforth that $s_1 \leq m_1$. Now suppose that $m_1 < s_i < m - m_n$ for some $s_i \in s$. Then form S with $|S| = s_i$ so that S includes A_1, excludes A_n, contains something in A_3 and does not contain everything in A_2. Then S is not sincere, but it is admissible by $C2$. Therefore, to avoid insincerity for s we require

$$s_i \leq m_1 \quad \text{or} \quad m - m_n \leq s_i \quad \text{for every } s_i \in s. \tag{3}$$

Now for any $s_i \leq m_1$ form S with $|S| = s_i$ with one candidate from A_2 and the rest (if any) from A_1. This S is insincere. To avoid admissibility for S by $C2$, it must be true that $s_i < s_j \leq m_1 + m_2$ for some $s_j \in s$. However, (3) also requires $s_j \leq m_1$ to avoid insincerity for s. Therefore, it follows that if s is to be sincere for P, then for every $s_i \leq m_1$ there is an $s_j \leq m_1$ such that $s_i < s_j$. Since $s_1 \leq m_1$, this is impossible unless m_1 is infinite. But m_1 is finite. Therefore s is not sincere for P.

THEOREM 4. *System* s *is strategyproof for every dichotomous* P *if and only if* s *is the approval voting system. If* P *has more than two indifference subsets then no* s *is strategyproof for* P.

Proof. When P is dichotomous, with indifference subsets $M(P)$ and $L(P)$, it follows from Corollary 2 that approval voting is strategyproof for P. Suppose then that s is not the approval voting system. If $i \in s$, $1 \leq i < m-1$ and $i+1 \notin s$, let P be dichotomous with $|M(P)| = i+1$. Then, by $C2$, every S that has $|S| = i$ and $S \subset M(P)$ is admissible for s and P, and therefore s is not strategyproof for P. On the other hand, if $i \in s$, $1 < i \leq m-1$ and $i-1 \notin s$, let P be dichotomous with $|M(P)| = i-1$. Then, by $C1$, every S that has $|S| = i$ and $M(P) \subset S$ is admissible for s and P, and therefore s is not strategyproof for P.

Suppose next that P has indifference subsets $A_1, A_2, \cdots, A_n$ with $n \geq 3$, and let $m_i = |A_i|$ with $m_1 + m_2 + \cdots + m_n = m$. Let $s = \{s_1, \cdots, s_r\}$ with $1 \leq s_1 < \cdots < s_r \leq m-1$. If $s_1 > m_1$ then, by $C1$, every S with $|S| = s_1$ and $M(P) \subset S$ is admissible, and there is more than one such S. If $s_1 < m_1$ and $s_i \neq m_1$ for all $s_i \in s$, let s_j be the largest s_i that is less than m_1. Then, by $C2$, all S with $|S| = s_j$ and $S \subset A_1 = M(P)$ are admissible. Hence if $s_i \neq m_1$ for all $s_i \in s$ then there are at least two admissible strategies, and s is not strategyproof for P.

Continuing in the context of the preceding paragraph, if $s_r < m - m_n$ then, by $C2$, every S with $|S| = s_r$ and $S \cap A_n = \varnothing$ is admissible for s and P, and there is more than one such S. If $s_r > m - m_n$ and $s_i \neq m - m_n$ for all $s_i \in s$, let s_k be the smallest s_i that exceeds $m - m_n$. Then, by $C1$, every S with $|S| = s_k$ and $A_1 \cup A_2 \cup \cdots \cup A_{n-1} \subset S$ is admissible, and there is more than one such S. Hence if $s_i \neq m - m_n$ for all $s_i \in s$ then s is not strategyproof for P.

It then follows from the two preceding paragraphs that, if s is to be strategyproof for P, we must have m_1 and $m - m_n$ in s. However, if this is so, then $M(P) = A_1$ and $A_1 \cup A_2 \cup \cdots \cup A_{n-1}$ are both admissible for s and P according to Theorem 2. Hence no s can be strategyproof for P when $n \geq 3$.

THEOREM 5. *If all preference orders in* V *are dichotomous and* s *is the approval voting system, then* $F(\alpha) = \mathrm{Con}(V)$ *for all* $\alpha \in V(s)$.

Proof. Given the theorem's hypotheses, Corollary 2 implies that $V(s)$ consists of the unique function that assigns the subset of most-preferred candidates to each order in V. The outcome of this function must be Con(V) since as many terms in V have a preferred to b as have b preferred to a if, and only if, a gets as many votes as b.

THEOREM 6. *Suppose* s *is not the approval voting system. Then some* V *consisting entirely of dichotomous preference orders has an* $\alpha \in V(s)$ *such that* $F(\alpha) \cap \mathrm{Con}(V) = \varnothing$.

Proof. Assume that s is not the approval voting system and let k_0 be an integer in $\{1, 2, \cdots, m-1\}$ that is not in s. Let V be a list of dichotmous preference orders such that for each P in this list $M(P)$ contains exactly k_0 candidates and $L(P)$ contains $m - k_0$ candidates. Then let $k^* \in s$ be such that there are admissible strategies for each P in V that contain exactly k^* candidates. Such a k^* must exist since it is not possible to have every strategy inadmissible for system s. If $k^* < k_0$ then any strategy for a voter that consists of k^* of the voter's most-preferred candidates will be admissible. And if $k^* > k_0$ then any strategy for a voter that contains the voter's k_0 most-preferred candidates and any $k^* - k_0$ of the voter's least-preferred candidates will be admissible. We now consider two cases according to whether $k^* < k_0$ or $k^* > k_0$.

Case 1: $k^* < k_0$. Construct V in the dichotomous format described above in such a way that candidate a is a most-preferred candidate for every voter and no other candidate is a most-preferred candidate for every voter. Then $\mathrm{Con}(V) = \{a\}$. Let $\alpha \in V(s)$ be such that a is never in the subset of k^* most-preferred candidates assigned to each voter by α. Then $F(\alpha)$ will not contain a.

Case 2: $k^* > k_0$. Construct dichotomous V in such a way that candidate a is in every voter's least-preferred subset and, with $a_1, a_2, \cdots, a_{m-1}$ the other $m-1$ candidates, a_i is a least-preferred candidate for the ith term in P. Then $a \notin \mathrm{Con}(V)$, and with $\alpha \in V(s)$ such that a is in the subset of k^* candidates assigned to each voter by α and a_i is not in the α-assigned subset of k^* candidates for the ith voter $(i = 1, \cdots, m-1)$, $F(\alpha) = \{a\}$. This completes the proof of the theorem.

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Stability, Coalitions and Schisms in Proportional Representation Systems*

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Methods to allocate seats in proportional representation systems are investigated in terms of underlying common-sense properties. Important among these are concepts of stability, coalition encouragement and schism encouragement. In addition, a new concept of uniformity is introduced which seems inherent in the very idea of the word "method," and it is shown that this concept is essentially equivalent to a previously investigated property called consistency. These and other criteria are shown to uniquely determine certain methods. In particular, the Jefferson method (incorrectly credited to d'Hondt) and the Quota method are given characterizations which commend them as the principal candidates for use in PR systems.

1. Introduction

There exist wholesale numbers of possible election procedures. A basic classification has been made of these into "plurality systems" and "proportional representation systems."

In a *plurality system* an elector usually casts one vote for the candidate or the (party) list of candidates of his or her choice in some election district, and the candidate or list receiving a majority or plurality is elected. Such systems are based on a notion of geographical representation. Mid-nineteenth century Europe saw an increasing dissatisfaction with plurality systems as unfair to minorities, for small political parties were effectively barred from having any representation whenever their adherents were distributed throughout many single-member election districts.

This led to the idea of *proportional representation* which, in its pure form, has electors cast one vote for a party or party list in a multi-member district and then, by some rule, metes numbers of seats "proportionally" among the parties according to their respective vote totals. Of course, variants of both types of system exist, as do complex mixtures of both.

This article is focused on the pure form of the *proportional representation problem:* voters cast a single vote for a party in a multi-member district and the question is to determine the *just* number of representatives due each party. Exact proportionality cannot, in general, be achieved since representation must be integral. Some "rounding" must take place. Appearances to the contrary, an operational definition of exactly how to effect this rounding is not easily forthcoming and history is rich with controversies over proposed solutions and methods.

We argue that the only valid approach to the comparison between methods, and hence to the ultimate choice of a method, is through a careful analysis of their *properties.* Consider an arbitrary method of allocation M. Does M have the property of always assuring a party at least its exact proportional number of seats rounded down, or at most that number of seats rounded up? Suppose that, with vote totals unchanged, the number of seats in a parliament is increased: does the method M always assure each party at least as much representation as was allocated to it before? Suppose that, instead of standing alone, two parties form a coalition which obtains the same vote total as the sum of the votes that would have been received by the parties separately: does the method M give at least as many seats to the coalition as the sum of the seats M would have allocated to the parties separately? Postulate an election in which some subset of parties, each standing

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alone, is allocated certain numbers of seats by a method M and that they jointly hold h seats: if M were used to allocate those h seats among *only* these parties with the given vote totals, would M arrive at the same allocation?

It is perhaps surprising—but it is a fact—that *no* method enjoys many of the combinations of the properties suggested by these questions, that some methods are the *only ones* which enjoy certain combinations, and that some other combinations define particular classes of methods. Below, we describe the fundamental properties of allocation methods in the context of methods which have been advanced over the years since the problem of proportional representation first arose. We point to some impossible combinations, and characterize two methods which seem particularly apt for PR systems. In particular, we introduce the notions of methods being stable, encouraging coalitions and encouraging schisms. But, the choice of appropriate properties depends upon the particular situation, nation and heritage. Thus the moral of this article is: politicians should not choose numerical solutions, nor even numerical methods. Rather, they should argue the merits of properties of methods, and let their conclusions in principle determine the methods and thus the numbers. Thus, axiomatization finds its political role.

2. Background

Let us consider s parties, and represent the vote total of each party i by p_i, $1 \leqslant i \leqslant s$. A non-negative integer, h, will be the total number of seats to be allocated (called the *house size*). We only consider parties which receive votes, so that $p_i > 0$. For a given house size h, the problem is to find an *allocation* for h: namely, s non-negative integers $a_1, a_2, \ldots, a_s$ whose sum is h. A *solution* of the proportional representation problem is a function **f** which gives, for all possible vote totals $\mathbf{p} = (p_1, p_2, \ldots, p_s)$ and house size h, an allocation for h: $a_i = f_i(\mathbf{p}, h) \geqslant 0$, $\sum_i a_i = h$. A *method*, M, is the family of all solutions produced by a specific computational rule (this allows for the possibility of ties).

In fact a similar problem arises under plurality systems, but in a different guise. For, usually, a country is divided into states or provinces and each single-member election district is wholly contained in one such subdivision. How many election districts or representatives should one geographical region be allocated? This problem is known as the *apportionment problem.* (Of course, geographical apportionment can arise in proportional representation systems too.) In this case the numbers $p_1, p_2, \ldots, p_s$ are interpreted as the populations (or numbers of voters) in the various states, and a_i is the number of seats allocated to state i. The apportionment problem has a long and colorful history, particularly in the United States (Balinski and Young, 1974, 1975; Schmeckebier, 1941).

For proportional representation, three principal methods seem to have been considered: Sainte-Lagüe's (1910), d'Hondt's (1878, 1882), and Hamilton's (1966), usually known as *"la répartition au plus fort reste."* In the apportionment literature Sainte-Lagüe's and d'Hondt's have found their places (see Huntington, 1928), under other names, in a class of five so-called "modern" (Bliss, Brown, Eisenhart and Pearl, 1929; Morse, von Neumann and Eisenhart, 1948) methods which, from about 1920 through 1974, were the ones collectively considered for apportioning the United States House of Representatives.

The five "modern" methods were first grouped by E. V. Huntington (1921) via an approach to allocation based on pairwise comparisons of "inequality in representation." Given vote totals $\mathbf{p} = (p_1, \ldots, p_s)$ and an allocation $\mathbf{a} = (a_1, \ldots, a_s)$ for h consider the numbers p_i/a_i and a_i/p_i. These represent the number of votes per representative of party i and the number of representatives per vote of party i. If $p_i/a_i > p_j/a_j$, or $a_i/p_i < a_j/p_j$, or $a_j > a_i(p_j/p_i)$, or $\ldots$, $(p_i/a_i)(a_j/p_j) > 1$, then party j is *better off* than party i. Given a particular measure of inequality between a pair of parties such as $|p_i/a_i - p_j/a_j|$ or $|a_i/p_i - a_j/p_j|$ it is natural to ask whether the amount of inequality can be reduced by a transfer of one seat from the better-off party to the less-well-off party. For a given measure an allocation is said to be *in equilibrium* if no transfer of a representative from a "better off" party to a "worse off" party reduces the amount of inequality between them. Of course, certain conceivable measures may not (and do not) admit equilibrium solutions for all vote-total distributions, but Huntington showed (1921, 1928) that five measures do.

All of Huntington's methods are examples of the following type, called *divisor methods.* Let $d(a)$ be some monotone-increasing function of the non-negative integers a where $d(0) \geqslant 0$. The *divisor method* M *based on* $d(a)$ is defined to be the set of all solutions obtained in the following manner for successive house sizes h. For $h = 0$, the allocation must be zero for every party. Given that an allocation $(a_1, \ldots, a_s)$ has been found for a house size h, an allocation for house size $h + 1$ is found by giving one more seat to a party k for which $p_k/d(a_k)$ is a maximum.

If there are several states that are tied for maximum, then several allocations may result unless some tie-breaking rule is employed.

The numbers $p_i/d(a_i)$ may be thought of as measuring the "priority" of a party with a_i seats to receive one more seat. Thus, if one more seat is to be distributed, then party i will "deserve" it more than party j if $p_i/d(a_i) > p_j/d(a_j)$. In this context, $d(a_i)$ may be thought of as some sort of "weighting" of the number of seats the party i already has.

One of the most commonly used methods for PR, e.g., in Argentina, Belgium, Brazil, Finland, Israel, Liechtenstein, and the Netherlands (Inter-Parliamentary Union, 1976), is that of *d'Hondt,* a nineteenth-century Belgian lawyer and proportional representation advocate. In fact the same method is known in the literature variously as the method of "highest average" or *"la répartition à la plus forte moyenne"* and is described differently, though in fact it leads to the same result (see section 4 below). This method was actually first proposed by Thomas Jefferson in 1792 and has therefore been called the *Jefferson method* **J** (Jefferson, 1904; Balinski and Young, 1975).

Table 1. Huntington's Five Methods

Method	Divisor Criterion $d(a)$
Smallest Divisors (SD)	a
Harmonic Mean (HM)	$2a(a+1)/(2a+1)$
Equal Proportions (EP)	$(a(a+1))^{1/2}$
Webster (W) or Major Fractions or Sainte-Lagüe Formula	$(a+\frac{1}{2})$
Jefferson (J) or Greatest Divisors or d'Hondt or Plus Forte Moyenne	$a+1$

The Jefferson method is one of the five divisor methods studied by Huntington, and it uses the divisor criterion $d(a) = a + 1$. The rationalization for this particular criterion is as follows. If party i has a_i seats then p_i/a_i—the number of votes per seat—is a measure of how well represented that party currently is. If one *more* seat were available for distribution, the Jefferson method proceeds by giving the extra seat to a party that would be worst off were *every* party to get one more seat, i.e., by giving the extra seat to a party k with the largest $p_k/(a_k+1)$.

Another plausible criterion, one might argue, would be always to give the "additional" seat to the party that is *currently* worst off, i.e., to the party for which p_k/a_k is a maximum. This method is known in the apportionment literature as the "method of smallest divisors" (SD), and is also one of Huntington's five. These five methods, and their corresponding divisor criteria, are shown in Table 1.

That these are actually all different methods is seen from the example in Table 2, where the five methods allocate 36 seats among six parties in five different ways.

Another commonly used rule, e.g., in Denmark and Norway (Inter-Parliamentary Union, 1976), is that known as Sainte-Lagüe's method (fourth in the list of Huntington's methods). This is also known in the apportionment literature as "the method of major fractions," but was in fact first suggested in embryonic form in 1832 by Daniel Webster (1903) and has therefore been called the *Webster method* **W** (1975). A particular variant of W is the modified "method of odd numbers" used in Sweden. It is defined by: $d(0) = 7/10$ (instead of $d(0) = \frac{1}{2}$), and otherwise the divisors are identical with those of W, $d(a) = a + \frac{1}{2}$. The third of Huntington's methods, Equal Proportions (EP), was the one favored by Huntington, and is the method currently used to apportion the United States House of Representatives.

It is an interesting historical note that Sainte-Lagüe (1910) came upon the Webster

Table 2. Example Showing Distinctness of Huntington's Five Methods

Party	Votes Received	Exact Proportionality	SD	HM	EP	W	J
A	27,744	9.988	10	10	10	10	11
B	25,178	9.064	9	9	9	9	9
C	19,947	7.181	7	7	7	8	7
D	14,614	5.261	5	5	6	5	5
E	9,225	3.321	3	4	3	3	3
F	3,292	1.185	2	1	1	1	1
	100,000	36.000	36	36	36	36	36

method quite independently via the idea of minimizing a total measure of the inequality of an allocation. He proposed that an allocation should minimize (here $p = \sum_i p_i$)

$$\sum_i p_i \left(\frac{h}{p} - \frac{a_i}{p_i}\right)^2$$

since in a perfect allocation $h/p = a_i/p_i$ for all i. The Webster method provides solutions which do this. In the same paper Sainte-Lagüe suggests in words (though not in symbols) that one could be interested in minimizing

$$\sum_i a_i \left(\frac{p}{h} - \frac{p_i}{a_i}\right)^2$$

but that "one is led to a more complex rule." In fact, this gives precisely the method of Equal Proportions.

There is another method that is often proposed for proportional representation, and which is, seemingly, the most natural one. Although known by several names, including *"la répartition au plus fort reste"* and "Vinton's method of 1850," it was apparently first proposed by Alexander Hamilton in 1792 and has therefore been called the *Hamilton method* **H** (1966; Balinski and Young, 1975).

Define the *exact quota* of party j to be $q_j = q_j(\mathbf{p},h) = p_j h/\Sigma_i p_i$; it is the exactly proportional number of seats deserved by party j and the number that one would wish to allocate to j were it integral. Let $\lfloor q_j \rfloor$ denote the largest integer less than or equal to q_j. The *Hamilton method* is defined in the following way: first, give to each party j $\lfloor q_j \rfloor$ seats; then order the parties by their fractional remainders $d_j = q_j - \lfloor q_j \rfloor \geqslant 0$ in a priority list $d_{j_1} \geqslant \ldots \geqslant d_{j_s}$. Second, give one additional seat to each of the first $h - \Sigma \lfloor q_j \rfloor$ parties on the list. If there are ties then there exist distinct arrangements of the priority list and, hence, possibly several solutions.

The rationale of the Hamilton method is the following. Every party j certainly deserves a number of seats at least equal to its *lower quota* $\lfloor q_j \rfloor$, hence we begin by giving each party at least this many seats. Any method such that

$$a_j \geqslant \lfloor q_j \rfloor = \lfloor p_j h/p \rfloor$$

for all allocations **a** for h is said to *satisfy lower quota*. By the same token, no party j deserves to receive more than $\lceil q_j \rceil$ seats (where $\lceil q_j \rceil$ is the next integer larger than q_j, or if q_j itself is integer, $\lceil q_j \rceil = q_j$). $\lceil q_j \rceil$ is called the *upper quota* of state j, and a method *satisfies upper quota* if for all allocations **a** for h,

$$a_j \leqslant \lceil q_j \rceil = \lceil p_j h/p \rceil.$$

A method M *satisfies quota* if it satisfies both lower and upper quota for all parties j and allocations **a**. In particular, the Hamilton method satisfies quota.

The Hamilton method, or modifications of it, is currently used for PR in the parliaments of Costa Rica and Italy (Inter-Parliamentary Union, 1976), and was formerly used in Israel. Under the name of the *Vinton method of 1850* it was also used to apportion the United States House of Representatives from 1850 to 1900.

But in the United States experience with the Hamilton method a startling discovery was made: in 1881 it would have given Alabama 8 seats in a house of 299 but only 7 seats in a house of 300. "This atrocity which [mathematicians] have elected to call a 'paradox' . . . this freak presents a mathematical impossibility" (Rep. John C. Bell of Colorado, 8 January 1901, *Congressional Record*, Vol. 34, pp. 724–25), is a typical, albeit somewhat hysterical, politician's reaction to the property. The phenomenon, known as the *Alabama paradox*, is not an isolated quirk of the Hamilton method, but occurs frequently. Consider the vote totals of the six parties in Table 3: party E is given 4 seats when 37 are to be allocated, but only 3 when 38 are to be allocated. The point is not that one would necessarily be in a situation where the number of seats to be allocated increases by 1—although history is replete with houses of representatives which increase their numbers—but rather that any method exhibiting such behavior runs totally contrary to any notion of what a fair and reasonable method should do. One need only refer to the universal reaction of shock among politicians when the event occurred to gauge the *political* unacceptability of the Hamilton method. Its use in the United States has been definitely abandoned. Were a political *party* to lose a seat only because the total number of seats to be distributed were increased, a similar hue and cry would certainly result.

Thus, an essential property for any fair and reasonable PR method M is that it be *house monotone*, that is, for any M-solution **f**, no party must receive fewer seats if the number of seats to be allocated increases:

$$\mathbf{f}(\mathbf{p},h+1) \geqslant f(\mathbf{p},h) \quad \text{for all } \mathbf{p} \text{ and } h.$$

It was the search for house monotone methods that motivated E. V. Huntington, beginning in the early part of this century, to formulate the class of five methods exhibited in

Table 1. All of these methods are house monotone, as indeed are *all* divisor methods.

3. Stable Methods

Parties in proportional representation systems are dynamic. They may group together for electoral purposes, but they also may splinter. The properties of the method used for allocating representation to parties may well have consequences for this tendency to coalesce or to splinter—in short, for their *stability* (Rokkan, 1968). Specifically, it is pertinent to ask how the number of seats allocated by a method **M** to the joint vote total of two parties coalesced into one, compares with the seats allocated by **M** to the two parties separately.

This is independent of any coalition or schism formation which might occur in a parliament *after* an election. Rather, it forms an underlying structural incentive for larger or smaller parties to form *before* elections. This type of incentive is institutional rather than psychological and cannot be expected to be directly observable or measurable in any one election. Nevertheless, it constitutes a normative basis for the design of proportional representation systems.

Consider a method **M** and a situation with vote totals **p** in which some party has p^* votes and is allocated a^* seats and another party has $\bar{p}$ votes and is allocated $\bar{a}$ seats. Suppose now that the two parties merge into one party with combined vote total $\bar{p} + p^*$. The method **M** is said to be *stable* if there is an **M**-allocation of seats that gives the merged party *no more* than $a^* + \bar{a} + 1$ seats and *no less* than $a^* + \bar{a} - 1$ seats. The meaning of this condition is that the allocations to parties separately should not be too different from what the parties would receive by merger, for if this is not the case then the method itself may tend to encourage party mergers or schisms, i.e., instability.

The following result may then be shown (Balinski and Young, 1978a).

(1) Any divisor method with divisor criterion d(a) *satisfying*

$$d(a_1 + a_2) \leqslant d(a_1) + d(a_2) \leqslant d(a_1 + a_2 + 1)$$

is stable.

It may be checked that the five methods of Table 1 satisfy the condition; hence all of them are stable.

To see how stability works, consider the vote totals of the six parties in Table 2, and suppose that parties D and E merge to form one party. The Jefferson method then accords the merged party one more seat than the sum of their separate allocations. On the other hand, consider the seemingly similar divisor method based on $d(a) = a - 1$. This does not satisfy the condition (1), and the example of Table 3 shows it is unstable: the method allocates to the merged party a total of 2 seats *less* than the sum of their separate allocations.

There are non-divisor methods which are stable, for example it is proven in the appendix that:

(2) The Hamilton method is stable.

4. Methods Encouraging Coalitions and Methods Encouraging Schisms

For proportional representation it is important to ask, not only whether a method is stable, but also whether it tends to encourage parties to merge or to splinter. In other words the way in which a method allocates seats may

Table 3. Illustration of the Alabama Paradox

Party Vote Total	A 27,744	B 25,178	C 19,947	D 14,614	E 9,225	F 3,292	Totals 100,000
Exact quota 35	9.710	8.812	6.981	5.115	3.229	1.152	35
H allocation 35	10	9	7	5	3	1	35
Exact quota 36	9.988	9.064	7.181	5.261	3.231	1.185	36
H allocation 36	10	9	7	5	4	1	36
Exact quota 37	10.265	9.316	7.380	5.407	3.413	1.218	37
H allocation 37	10	9	7	6	4	1	37
Exact quota 38	10.543	9.568	7.580	5.553	3.506	1.251	38
H allocation 38	10	10	8	6	3	1	38
Exact quota 39	10.820	9.819	7.779	5.699	3.598	1.284	39
H allocation 39	11	10	8	6	3	1	39

create a subtle institutional incentive for larger or smaller parties to evolve, depending on whether the merger of parties tends to result in a net gain or a net loss of seats. For *political* stability it would usually be considered desirable to have methods of allocation that encourage parties to merge, by assuring that this would never result in a net loss of seats, and might in fact result in an increase.

To make these ideas precise, consider any method **M** and any situation **p** in which some party has $\bar{p}$ votes and is allocated $\bar{a}$ seats by **M** and some other party has p^* votes and is allocated a^* seats by **M**. Suppose that the "star" and "bar" parties merge to create a party with a total of $p^* + \bar{p}$ votes. We say that **M** *encourages coalitions* if in any such situation there is an **M**-allocation giving at least $a^* + \bar{a}$ seats to the coalesced party. Similarly, if there is an **M**-allocation giving at most $a^* + \bar{a}$ seats to the coalesced party then **M** is said to *encourage schisms.*

Consider, for example, the divisor method with divisor criterion $d(a) = a + 6$ applied for 36 seats to the vote totals given in Table 2, and compare this with the results of the Jefferson method. If parties B and C merge, then they receive *four* more seats under the $(a + 6)$ method, and one more under Jefferson. Thus under both of these methods there is an *incentive* for parties to merge. However, the $(a + 6)$ method is unstable. Moreover, it will be noticed that, while the $(a + 6)$ method works even more strongly than **J** to encourage coalitions it does so at the cost of seriously penalizing the smaller parties. Thus, *none* of the three smallest parties gets even its lower quota under the $(a + 6)$ method, whereas all of them do so under **J**. Since any party certainly has a very strong claim in equity to be allocated at least its lower quota, the Jefferson method seems to be the superior one of the two. In fact, we have the following result.

(3) The Jefferson method is the only divisor method that satisfies lower quota and encourages coalitions.

This is a consequence of more general results proved elsewhere (Balinski and Young, 1978a, 1978c).

Viewed in this light, the Jefferson method presents strong credentials for being adopted in a proportional representation system. Sainte-Lagüe appears to have realized the tendency of **J** to encourage coalitions, but he gave no proofs and his statement has the curiosity of referring to a comparison: "In comparing the two rules, one can show that the d'Hondt rule (**J**) favors the grouping of parties which, by coalescing, may receive more seats; whereas the method of least squares (**W**) favors neither groupings nor schisms" (1910, p. 378). A unique characterization of **W** may, however, be given in terms of its

Table 4. J Compared with an Unstable Method

Party	Vote Totals Before Merger	Vote Totals After Merger	J-Solution Before	J-Solution After	$(a-1)$-Solution Before	$(a-1)$-Solution After
A	27,744	27,744	11	10	9	10
B	25,178	25,178	9	9	8	9
C	19,947	19,947	7	7	7	7
D	14,614	23,839	5	9	6	8
E	9,225		3		4	
F	3,292	3,292	1	1	2	2
	100,000	100,000	36	36	36	36

Table 5. Example of Encouraging Coalitions

Party	Vote Totals Before Merger	Vote Totals After Merger	Exact Quotas Before	Exact Quotas After	J-Solution Before	J-Solution After	$(a+6)$-Solution Before	$(a+6)$-Solution After
A	27,744	27,744	9.998	9.998	11	10	13	11
B	25,178	45,125	9.064	16.245	9	17	11	22
C	19,947		7.181		7		7	
D	14,614	14,614	5.261	5.261	5	5	4	3
E	9,225	9,225	3.321	3.321	3	3	1	0
F	3,292	3,292	1.185	1.185	1	1	0	0
	100,000	100,000	36.000	36.000	36	36	36	36

"rounding" properties (see Balinski and Young, 1977a).

The method of smallest divisors is, in a certain sense, "symmetric" with J. Thus we have the following result (which is also a consequence of theorems of Balinski and Young, 1978a, 1978c).

(4) The method of smallest divisors is the only divisor method that satisfies upper quota and encourages schisms.

5. Uniform Methods

A "method" of allocation brings to mind the dictionary definition: a systematic means or manner of procedure. It conveys a sense of regularity. What is a valid mathematical interpretation of this meaning? Here we introduce and explain a notion which captures this sense.

Consider a two-party problem in which each party has precisely the same number of votes and there are an even number of seats to allocate. We say that a method is *balanced* if it has an allocation giving the same number of seats to each party. (An equivalent definition is given in Balinski and Young, 1978a.) It is an obvious truism that every method ever proposed or considered is balanced. In the sequel we will only consider balanced methods—except to point to the existence of "unbalanced" ones—and so will drop any further (redundant) mention of this property.

Divisor methods (which are of course balanced) are house monotone. But they also have another property—called "uniformity"—which is a logical transcription of the intuitive notion of "method." Consider the allocation problem of Table 2, and suppose, for example, that the problem is modified as follows: parties E and F are disqualified, so the total vote is 87,483 and there are 32 seats among the remaining states. If the Webster method W were now used to allocate a house of 32 seats to the parties A, B, C, and D then one would naturally expect that the number of seats would be exactly the same as those in Table 2, namely, 10, 9, 8, and 5, respectively. That is, dropping certain parties and the seats allocated to them should not change the distribution of seats between the parties which remain. The solution might be said to be "secession-proof." One actually would expect more of a "method": namely, *whenever* four parties having the vote totals of A, B, C, and D are to share 32 seats among them, then these seats should always be shared in the same way (except allowing for ties). This expectation of "uniformity" is fulfilled by the Webster method, all divisor methods and, in fact, by a vastly larger class of methods.

To be mathematically precise, consider an arbitrary method M and two different problems having vote totals (p,q) and (p,q′), each problem containing a set of parties having identical vote totals p. Suppose that (a,b) is an M-allocation for (p,q), that (a′,b′) is an M-allocation for (p,q′) and $\Sigma a_i = \Sigma a_i'$. Then M is *uniform* if (a′,b) is also an M-allocation for (p,q) and (a,b′) is also an M-allocation for (p,q′).

Not all methods are uniform: the Hamilton method is not. For, consider the example of Table 3. The Hamilton method uniquely divides 10 seats between D, E and F by the allocation (5,4,1) in one instance (when the total house is 36), whereas it uniquely allocates them (6,3,1) in another instance (when the total house is 39). On the other hand, divisor methods are uniform (see statement (6) below) since they proceed by comparing the priorities of parties pair by pair through evaluation of $p/d(a)$, so the order in assigning seats to a subset of parties need not change in the presence of other parties. This observation immediately suggests a generalization.

Let $r(p,a)$ be any real-valued function of two real variables called a *rank-index* (possibly including $\pm\infty$ for certain values of p and a). Given a rank-index, a (generalized) *Huntington method* of allocation M is the set of all solutions f obtained recursively as follows (Balinski and Young, 1977b):

(i) $f_i(\mathbf{p},0) = 0, \quad 1 \leqslant i \leqslant s.$

(ii) if $a_i = f_i(\mathbf{p},h)$ is an M allocation for h, and k is some one party for which

$$r(p_k,a_k) \geqslant r(p_i,a_i) \text{ for } 1 \leqslant i \leqslant s,$$

then

$$f_k(\mathbf{p},h+1) = a_k + 1,$$
$$f_i(\mathbf{p},h+1) = a_i \text{ for } i \neq k.$$

Since we consider only balanced methods, we must have $r(p,a-1) \geqslant r(p,a)$ for all p and a, because otherwise an allocation to the two-party problem (p,p) of form $(a-1, a+1)$ would result. Huntington methods are a direct generalization of divisor methods and are house monotone and uniform for the same reasons. They admit a local characterization of allocations.

(5) **a** *is an allocation for the Huntington method* M *based on* r(p,a) *if and only if (with* r(p,−1) = ∞*)*

$$\max_i r(p_i,a_i) \leqslant \min_j r(p_j,a_j-1).$$

This is immediate by definition. It applies to divisor methods; thus, for example, **a** is a Jefferson allocation if and only if

$$\max_i p_i/(a_i + 1) \leqslant \min_i p_i/a_i.$$

One application of this result is to show that various methods that go by different names and descriptions in the literature are actually the same method. For example, the method of *la répartition à la plus forte moyenne*—usually ascribed to d'Hondt—has the following description (*La Grande Encyclopédie*, 1973). First, give to each state its lower quota $\lfloor p_i h/p \rfloor = a_i$, the number of seats remaining to be distributed is then $h - \Sigma_i a_i$. For these remaining seats, proceed as for **J**, (i.e., as for the Huntington method with $r(p,a) = P/(a+1)$). But this must give exactly the same answer as **J**, since as we have noted above in statement (3), any **J**-solution *automatically* satisfies lower quota.

D'Hondt actually made the following proposal: "to allocate discrete entities proportionally among several numbers, it is necessary to divide these numbers by a common divisor, producing quotients whose sum is equal to the number of entities to be allocated" (1882, p. 22). Thus, according to d'Hondt, we are to find a "divisor" λ such that the integer parts of the numbers p_i/λ sum to h, i.e., $\Sigma_i \lfloor p_i/\lambda \rfloor = h$. To see that this is the same as **J**, set $a_i = \lfloor p_i/\lambda \rfloor$ and notice that, by definition of the integer part,

$$a_i + 1 > p_i/\lambda \geqslant a_i.$$

Thus

$$p_i/(a_i+1) < \lambda \leqslant p_i/a_i \text{ for all } i,$$

i.e.,

$$\max_i p_i/(a_i+1) \leqslant \min_i p_i/a_i,$$

so by (5) $(a_1, \ldots, a_s)$ is a **J** apportionment for h. The converse is established similarly.

But exactly the same idea was proposed by Thomas Jefferson in a letter to George Washington nearly a century earlier (1792): "for *representatives* there can be no such common ratio, or divisor which ... will divide them exactly without a remainder or fraction. I answer then ... that representatives [must be divided] as *nearly* as the *nearest* ratio will admit; and the fractions must be neglected" (1904, p. 463). For this reason the divisor method **J** must be credited to Jefferson.

That Huntington methods constitute a very general class may be seen from the following result (which follows from statement (7) below and the main theorem of Balinski and Young, 1977b).

(6) A (balanced) **M** *is house monotone and uniform if and only if it is a Huntington method based on a rank-index* r(p,a) *which is non-increasing in* a.

House-monotonicity and uniformity are sufficient to imply the existence of a rank-index $r(p,a)$. This is a strong implication. It permits a host of pertinent conclusions. It should be noted that there do exist rather dubious non-balanced Huntington methods characterized by rank-indices such as $r(p,a) = a/p$: this method gives all seats to the party which receives the first seat!

The statement (6) permits a strengthening of some of the previous results. Thus, (3) can be stated: there is exactly one uniform, house monotone method satisfying lower quota and encouraging coalitions, namely, the Jefferson method. And, (4) can be reformulated: there is exactly one uniform, house monotone method satisfying upper quota and encouraging schisms, namely, the method of smallest divisors.

There is another way of viewing uniformity. Consider an arbitrary house monotone method **M**. The behavior of **M** can be specified simply by saying how one gets an allocation for a house with $h + 1$ seats *given* the allocation for a house with h seats; that is, all that is needed to be known is which state has priority in receiving the next seat. An arbitrary method **M** is *consistent* if the decision as to which party of any pair most deserves the extra seat when the house size is increased by 1, depends *only* upon the vote totals and the seats already allocated to those parties singly, and not on any other data (such as the house size, the number of other parties, or the vote totals of other parties). The example of Table 3 shows that the Hamilton method is not consistent. Parties A and E receive 10 and 3 seats, respectively, for $h = 35$ and $h = 38$, but uniquely receive 10 and 4 for $h = 36$ and uniquely 11 and 3 for $h = 39$. However, it is immediately clear that all Huntington methods are consistent. Indeed, consistency and uniformity are essentially the same:

(7) A (balanced) house monotone method **M** *is uniform if and only if it is consistent.*

For the proof of this result see the Appendix.

Unfortunately,

(8) There is no uniform, house monotone method which satisfies quota.

If a method is house monotone and uniform then it is a Huntington method (see Appendix). But the Jefferson method is the only Huntington method that satisfies lower quota and the method of smallest divisors is the only method that satisfies upper quota (Balinski and Young, 1975, 1978a). Since these methods are different, there can be no uniform, house monotone method satisfying quota.

6. Quotatone Methods

Three properties appear to dominate in importance—house monotonicity, uniformity, and satisfying quota—but they cannot be satisfied by any one method. This raises the question: is there some "minimal" weakening of these conditions which admits the existence of a method?

The need for house monotonicity has already been demonstrated. Allocations which do not satisfy quota—that is, which are not the result of rounding the exact proportional share of representation due to a party either up or down—seem to violate common sense and have proven to be politically subject to attack (see, for example, Rokkan, 1968; Webster, 1903). A party receiving 40 seats when its proportional share is 37.34 seems unreasonably well served; if its true share were instead 43.34, its leadership would justifiably complain. Satisfying quota seems essential. Note that any method satisfying quota is *almost stable* in the sense that if any two parties with a^* and $\overline{a}$ coalesce then the coalesced party receives b seats where $a^* + \overline{a} - 2 \leqslant b \leqslant a^* + \overline{a} + 2$.

Given these considerations, it is reasonable to attempt to weaken the uniformity or consistency condition. Specifically, we say that a method **M** is *quota-consistent* if it is consistent subject to the need to satisfy upper quota, that is, consistent unless this imperative is in conflict with satisfying upper quota, in which case it cedes to the latter stronger imperative. Then, it can be affirmed (Balinski and Young, 1974, 1975):

(9) There exists a unique allocation method **Q**, *the quota method, that is house monotone, quota-consistent and satisfies quota.*

The *quota method* **Q** is related to the Jefferson method, and may be described as the set of allocations computed recursively as follows:

(i) $f_i(\mathbf{p},0) = 0, \quad 1 \leqslant i \leqslant s.$

(ii) Let $a_i = f_i(\mathbf{p},h)$ be an allocation for h and let $E(h{+}1)$ be the set of parties which can be given an extra seat without violating upper quota at house size $h + 1$. If $k \in E(h{+}1)$ is some one party for which

$$p_k(a_k + 1) \geqslant p_i/(a_i + 1) \text{ for all } i \in E(h{+}1)$$

then

$$f_k(\mathbf{p},h{+}1) = a_k + 1,$$
$$f_i(\mathbf{p},h{+}1) = a_i \text{ for } i \neq k.$$

Statement (9) gives powerful reasons for acceptance of the quota method **Q** in problems of proportional representation. In practice one finds that **Q** has a tendency to produce solutions that round up the exact quotas of large parties more often than those of small parties. This seems reasonable for the application of **Q** to proportional representation systems in that it inferentially asks for a "large" vote before according any representation at all. Notice, however, that no large party is allowed more seats than its upper quota.

One can simply drop the uniformity or consistency condition and ask about the class of *all* methods that are house monotone and satisfy quota. These are called *quotatone methods*. They are describable and have been characterized in several different ways (Balinski and Young, 1978b; Still, 1976). There are a multiplicity of allocation methods which are quotatone and all operate as follows. In a house $h = 0$ all parties have no representation. Given an allocation $\mathbf{f}(\mathbf{p},h) = \mathbf{a}$ for h, the characterization specifies a set of parties which are "eligible" to receive an extra seat, "eligible" meaning that this can be done in a manner which will guarantee quota and house monotonicity. Some choice mechanism (e.g., a rank-index) can be imposed to determine which party receives the seat at $h + 1$ if there is more than one eligible party. The difficulty is that the set of eligible parties has no natural direct interpretation other than that just given, and that its computation is conceptually complex (although easily accomplished by computer).

In the class of quotatone methods the quota method **Q** enjoys an especially natural position (Balinski and Young, 1978b); moreover, its eligibility set is easily understood and computed since it simply asks that the upper quota condition not to be violated. It is the unique method which devolves from a minimal weakening of the consistency condition.

7. Conclusions

This article has addressed the problem of the allocation of integral representation to parties having vote totals in a proportional representation system. The principal point is that methods of allocation should not be chosen by bickering over numbers, nor, indeed, through ad hoc claims of various mechanical procedures, but rather by analysis of the properties of methods. The issue is to decide upon a method whose qualitative properties are equitable for the situation at hand. This analysis commends one of two methods: the quota method **Q** or the Jefferson method **J**.

The Jefferson method claims recognition because it is house monotone, uniform, guarantees lower quota, and encourages coalitions. Specifically, encouraging coalitions would seem to be precisely the type of stability desired for a body politic operating a proportional representation system. However, a major defect of **J** is that it fails the seemingly most commonsense test of satisfying quota. The Quota method merits recognition because it does satisfy quota, is consistent subject to that property, and is house monotone. It is "almost stable" (see Balinski and Young, 1978a), but does not necessarily encourage coalitions.

The choice between these methods of allocation should be made in terms of which *criteria* are viewed as most important for the situation in question.

Appendix

We first prove that the Hamilton method is stable (see Balinski and Young, 1977a).

Consider any two parties, say $i = 1,2$, and suppose that in a particular problem party i has an exact quota $q_i = n_i + r_i$, $n_i \geqslant 0$ and integer and $0 \leqslant r_i < 1$, and let a_i be their allocations at h. Then for the problem in which parties 1 and 2 form a coalition, its exact quota for h is $q_1 + q_2 = n_1 + n_2 + r_1 + r_2$. Let b be the number of seats given the coalition by the Hamilton method.

We consider several cases. First, if $b = n_1 + n_2$ then $r_1 + r_2 < 1$, implying that the same total number of parties is rounded up in both problems. If $r_1, r_2 > 0$ then it must be that $a_i = n_i$ $(i = 1,2)$. For otherwise one of the two parties would have a remainder r_i high enough in the list to warrant an extra seat while $r_1 + r_2 > r_1, r_2$ is not high enough, which cannot be. If $r_1 = 0$ then $a_1 = n_1$ and $a_2 = n_2$ or $a_2 = n_2 + 1$; in either case the criterion for stability is satisfied.

If $b = n_1 + n_2 + 1$, then since $a_i = n_i$ or $n_i + 1$ there is nothing more to show. If $b = n_1 + n_2 + 2$, then $r_1 + r_2 > 1$. Suppose stability is not satisfied, i.e., that $a_i = n_i$ $(i = 1,2)$. Then for some party $k \neq 1,2$, $r_1 + r_2 - 1 \geqslant r_k$ while $r_1 \leqslant r_k$ and $r_2 \leqslant r_k$. Thus $2r_k - 1 \geqslant r_k$ and $r_k \geqslant 1$, a contradiction.

Next, we show that a balanced house-monotone method **M** is uniform if and only if it is consistent (see Balinski and Young, 1975).

First, assume that **M** is house monotone and consistent. The main theorem of Balinski and Young (1977b) states that this implies **M** is a Huntington method based on some rank-index $r(p,a)$. If, in addition, **M** is balanced then $r(p,a-1) \geqslant r(p,a)$ and **a** is an **M**-apportionment if and only if the min-max inequality of (5) is satisfied. To prove uniformity suppose **(a,b)** is an **M**-apportionment for **(p,q)** and **(a′,b′)** is one for **(p,q′)** where $\Sigma_i a_i = \Sigma_i a'_i$, $\mathbf{a} \neq \mathbf{a}'$. Then, by (5),

$$\max_{i,j} \{r(p_i,a_i), r(q_j,b_j)\} \leqslant \min_{i,j} \{r(p_i,a_i-1), r(q_j,b_j-1)\},$$

(10) and

$$\max_{i,j} \{r(p_i,a'_i), r(q'_j,b'_j)\} \leqslant \min_{i,j} \{r(p_i,a'_i-1), r(q'_j,b'_j-1)\}.$$

It must be shown that **(a′,b)** is an **M**-apportionment for **(p,q)** and **(a,b′)** is an **M**-apportionment for **(p,q′)**. Since $\Sigma a_i = \Sigma a'_i$, $\mathbf{a} \neq \mathbf{a}'$ implies there exist parties k and l with $a_k \geqslant a'_k+1$ and $a_l \leqslant a'_l-1$. Therefore

$$r(p_l,a_l) \leqslant r(p_k,a_k-1) \leqslant r(p_k,a'_k) \leqslant r(p_l,a'_l-1) \leqslant r(p_l,a_l),$$

the first and third inequalities following from (10), the second and fourth following from $r(p,a)$ being non-increasing in a_i. Thus every inequality is an equation and the max-min inequalities (10) must also both be satisfied as equations having a common value, say λ. Therefore,

$$\max_{i,j} \{r(p_i,a'_i), r(q_j b_j)\} = \lambda = \min_{i,j} \{r(p_i,a'_i-1), r(q_j,b_j-1)\}$$

and

$$\max_{i,j} \{r(p_i,a_i), r(q_j,b'_j)\} = \lambda = \min_{i,j} \{r(p_i,a_i-1), r(q_j,b_j-1)\}$$

which establishes, by the test (5), the desired result.

Second, assume that **M** is house monotone and uniform (here it is not necessary to assume that **M** is balanced). Consider two problems, (p_1,p_2,**q**) and (p_1,p_2,**q**′), and suppose the first has an M-apportionment (a_1,a_2,**b**) and the second (a_1,a_2,**b**′), that the first has at the next house the apportionment (a_1+1,a_2,**b**), whereas the second has at the next house (a_1,a_2+1,**b**′), a seeming "switch" in priorities. Then, by uniformity, the first also has an apportionment (a_1,a_2+1,**b**) and the second also has (a_1+1,a_2,**b**) thus showing there is a "tie" in priorities and that **M** is consistent.

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The Structure of "Politics"

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Taxonomic definitions of politics *are chronically unable to extend class properties to the heterogeneous scope of political events, leading to the view that "politics" may be a standard cluster concept. Clusters of properties, however, may be arranged around* core *terms strongly retentive in ordinary uses of a concept. Some terms are even rigid designators, necessary and sufficient conditions for reference in all possible worlds. The concept of "politics" provides two* core *terms, "directiveness" and "aggregation," though not rigid designators. Such a* core *structure concentrates the standard cluster-analysis of "politics" on* extension, *not carrying over to all aspects of* sense, *thus permitting a weak and revised case for taxonomy on nonidentifying* core *terms. The implications of* core *terms in the concept of "politics" include the restriction of research-utility as an adequacy criterion and the acceptance of conventional status for distinctions between political and nonpolitical events.*

Taxonomic Definitions

"Economics" was once defined as the study of human behavior in conditions of scarcity (Robbins, 1935). Similar definitions, equally short and sweet, have recently been advanced for "politics." Politics is "the authoritative allocation of values" (Easton, 1953, 1965). Politics is "integration and adaptation . . . by means of the employment, or threat of employment, of more or less legitimate physical compulsion" (Almond, 1960). Politics is "authority patterns" (Eckstein, 1973). Though often these brief (and famous) phrases are arrived at after thoughtful and complicated deliberations, all assume that what "politics" *is* can be stated as a simple exercise in taxonomy: the identification of a restricted set of properties shared by all events describable as political.

The single-phrase, taxonomic definitions of "politics" are remarkably uniform on adequacy criteria. The dominant principle setting out what makes a definition satisfactory, even good, is research utility. Definitions of "politics" are to be judged by their usefulness in assisting students of politics to explain what it is they are studying. The early David Easton was motivated in part to define "politics" in order to define the discipline of political science, giving systematic shape to the profession by giving systematic shape to what is studied by those in the profession (Easton, 1953). Eckstein explicitly endorses the "subjective interests" of political scientists as the source of adequacy criteria for a definition of "politics" (Eckstein, 1973, p. 1158).

A common method of defining, plus widely shared adequacy criteria, might be expected to generate some happy consensus on specific definitions of "politics." Such has not been the case. Candidates for the defining property of "politics" have been subjected to withering fires of criticism, the hottest blasts frequently coming from friendly rivals in the taxonomic approach. Easton directed carefully timed blows at his predecessors in political inquiry. Eckstein dismisses Easton. In most cases, the proposed definition is faulted for inadequacies of coverage. Almond's reliance on force (like Weber's, earlier) ignores the occasional sovereignty of friendly persuasion and rule-permissive politics (Hart, 1961). Bargaining transactions, maximizing actions, criteria-establishing politics, seem among the events not covered by "authoritative allocation" (Mitchell, 1961; Frohock, 1974, 1977; Sorzano, 1977). Eckstein's own concession that symmetrical transactions are outside the pale of "authority structures" is a glum reminder of taxonomic mortality rates.

The easy success of critiques internal to the taxonomic method entitles one to ask if perhaps there is something wrong with the method itself. Recent work, drawing on the later Wittgenstein, has questioned the possibility of defining "politics" in taxonomic terms. It is maintained instead (Connolly, 1974) that "politics" is a cluster term, extending to a heterogeneous range of events loosely linked in a "family resemblance" way though without the single common property required for class terms. If "politics" is indeed a cluster term, then the chronic deficiencies of taxonomic definitions of "politics" are understandable. Each candidate for the defining property must be restricted to some members of the "politics" family, thus permitting critiques on inadequate coverage.

It is both refreshing and disturbing that the deep troubles of the taxonomic approach to "politics" can be so cleanly resolved. An entire generation of political theorists, almost a modern history of theory if Weber is included, is revealed as wrong, and even wrongheaded, in its basic method of defining. Such quick turns of fortune are the stuff of conceptual change, even new schools of thought. But since new schools of thought are often stillborn, one wants to ask if the dismissal is premature in its completeness. Developments in a number of areas, including language (Chomsky, 1972), anthropology (Levi-Strauss, 1963, 1966) and early-learning theory (Piaget, 1954, 1956) suggest that fixed structures are not strangers to human experience. Recent studies in philosophy have renewed the possibility of "essential" definitions (Kripke, 1972; Plantinga, 1974; Putnam, 1970, 1973, 1975). If fixed or rigid designators are respectable components of language, then at least the spirit, and perhaps parts of the flesh, of the taxonomic approach to "politics" may be resuscitable.

The purpose of this article will be (1) to inspect the brief for a cluster account of "politics" and (2) to suggest requirements for a structural conceptualization of "politics" which will (a) provide an alternative to the cluster account, and (b) reveal a drastically revised sense of taxonomic possibilities in political inquiry.

Cluster Concepts

"Family resemblance" is a powerful tool to explain many linguistic puzzles. Originally, it was used to dispel the notion that things describable with a common word had to have a common property, a notion which ordinary language does not support. Wittgenstein (1953) used "games" to make the point: "For if you look at them [games] you will not see something that is common to *all*, but similarities, relationships, and a whole series of them at that. . ." (p. 31). Then: "as in spinning a thread we twist fibre on fibre. And the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres" (p. 32). So common names need not denote a class property, but only a grouping of properties no one of which is found in all of the things the word describes.

Recent cluster theories of reference are developed on the idea of "family resemblance." Cluster theories deny that any single property can be isolated to identify the reference of a name, for no one property can be viewed as essential for successful reference. For example, if we identify Socrates as the teacher and friend of Plato, then a counterfactual situation where Socrates happened not to teach or even to know Plato would be one where the name "Socrates" has no reference; and this is clearly absurd, for it makes one, albeit important, fact about Socrates—that he taught Plato—an essential property when it seems undeniable that Socrates could have been Socrates had he not taught at all. Proponents of cluster theories maintain that a group of properties is found in successful reference. No single member of the group does the job alone (Searle, 1958).

Terms which refer to clusters of properties are common enough in ordinary language. "Lemon," for example (Scriven, 1959): yellow in color, ovoid shape, acid taste, a certain size and hardness, skin of a certain texture, grown on trees, originating in sun-belt states, and so on. Any object with all of these properties is a lemon. But lemons may also lack one or more of these properties and still be lemons, as would happen if fruit growers bred a pink lemon. Some cluster of properties seems decisive, not any one property. Or take certain diseases, like Hodgkins disease, in which a cluster of associated symptoms currently constitutes the disease, and no one can be certain that a single underlying property will be discovered (Putnam, 1975). Houses, trees, bread, horses—all seem to be aptly fitted to cluster theories.

The language of political discourse also seems to have cluster reference rather than essential properties. *Democracy*, for example: distribution of power, economic parity, widespread participation in politics, open access to power, dissemination of information, the capacity of a system to respond to citizen preferences, civil liberties and rights, and so on. Any political system which has all of these properties is clearly democratic. But the absence of one or more properties is still consistent with democratic arrangements. Or take *racism*, yet another disease with a cluster of associated symptoms no one of which is a necessary and sufficient condition for the phenomenon. Authority, representation, persuasion—many concepts of social life are spread across a variety of properties, no one of which seems to be present in all events describable by the term.

Some have maintained that "politics" is itself a cluster concept (Connolly, 1974). *Politics*, for example, can describe: commands, bargaining, institutional change, legal enactments, extraction of moneys, distribution, production, and so on. Any social system which contains all of these features has a political system, though the absence of one or more of these features is not decisive. If "politics" is a cluster concept, then the efforts of those seeking a taxonomic definition are in vain. The logic of a cluster account requires that no single property is essential for referential use of a term, for no property is universally distributed in all things we are warranted in describing with a cluster term. "Politics" is sometimes a form of command (power, authority); at other times it is a bargaining transaction; occasionally it involves institutional modification with no direct contact among agents, and so on. All of these things (and more) are describable as "political." Yet no common property seems to extend through all of them.

An added dividend is drawn from the cluster account of "politics." If a heterogeneous population of events is describable as political, then an emphasis on one aspect or another of this population may indicate a value-set with fundamental status. Marxists, for example, are inclined to describe institutions like the family as political. Intellectual descendants of John Stuart Mill resist such wide extensions of "politics," preferring to reserve the term for more formal government actions (occasioned by the famous "harm to another" criterion). Since the grammar of "politics," or its descriptive power, is wide and various, disputes (Marx versus Mill) over the extension of the term must be value disputes (not linguistic or "factual"). In that appealing phrase, *politics* may be an "essentially contested" concept (Gallie, 1955; McIntyre, 1973; Connolly, 1974). The proper use of the concept is endlessly disputable; and the rationale for grouping some of the events under the term while excluding others is a function of one's evaluative (moral?) perspective (Connolly, 1974).

A tidy package emerges. (1) "Politics" extends to events sufficiently heterogeneous to set aside taxonomic properties. (2) The extension of "politics" to such a diverse family of events provides for multiple "meanings," or senses, of the term. (3) Variation in the extension of "politics" is a function of alternative evaluative perspectives. Thus, taxonomic efforts at defining are dealt a mortal blow at the same time that champions of the sociology of knowledge are given new life: fundamental value-sets are the origin of differences over the descriptive use of those political concepts extendable to clusters of events. "Politics" is among these concepts.

Fortunately, the package can be unpacked for more lengthy scrutiny of its contents. Proposition (3) can be set aside here as an interesting use of cluster theories of reference. Proposition (1) is conceded: "politics" does extend to a heterogeneous cluster of events. The issue to be addressed here is whether the acceptance of (1) requires the acceptance of (2). Does a heterogeneous extension of "politics" require a heterogeneous intension, or sense, of the term? Is the *meaning* of politics, the aim of definitions in the taxonomic tradition, exhausted by its exasperatingly uneven descriptive scope?

Some Modifications on Cluster Theory

"Clusters" are, ironically, cluster-words. Several disparate properties of clusters provide for a grouping of items all describable as a cluster without yet the saving grace of a uniform property. Consider the following ways of grouping properties.

(1) The standard, and original, view of a cluster is a family of properties no one of which is essential for reference. Some of the cluster is required, though it is difficult to legislate how much. *Racism,* for example: a congeries of conditions and attitudes, not a single property of a society or an individual.

(2) A cluster of properties may be needed in its entirety for successful reference, and no single property is adequate. *Freedom,* for example: the presence of alternatives, the capacity for choice, the lack of restraints, the absence of penalties, and so on. A satisfactory list of properties for the concept of "freedom" may group conditions without a common thread, but the absence of any one defeats a referential use of the term. A situation where, for example, strong penalties attach to certain options is typically considered to be *not* one of freedom even if all other conditions in the conceptual list are present. Each and every one of the properties of "freedom" may be necessary for reference, though no one of the properties is sufficient in and of itself.

(3) Some one property in the cluster may be necessary for reference, though no more than one. *Legitimacy,* for example: charisma, tradition, legality (Weber); natural selection, chance, trial by combat (Spencer); historical determinism (the Stoics); and so on. What makes social arrangements, things as they are, "legitimate" is the issue in heavyweight contests among competing philosophies, not trim disputes between linguistic debaters. Even within philosophies, one or another property frequently rules out rival properties. A charismatic legitimacy is, ipso facto, not possible in a

traditionally legitimate society, for example. The cluster of properties permits the use of, usually, one property per referential function of the term. Such a cluster is of course not at all what Wittgenstein had in mind, and may be more accurately seen as a collection of different interpretations (perhaps even different senses, though this is uncertain) of the concept. Still, words like "legitimacy" are part of political discourse, apply to heterogeneous events, are essentially contested at the very highest levels, consist of a grouping of properties, and so on. They are not, however, standard cluster concepts like "racism."

(4) A cluster of properties may be heterogeneous in the standard sense, but some deeper connection may tie the properties to a common source. *Authority*, for example: right to command, intellectual influence, title to be believed (*O.E.D.*); legal entitlement, and so on. The use of "authority" may function on a standard cluster of such properties. But tracing the genealogy of the word may reveal a common source, say being an "author" or "original observer" of an event (Friedman, 1973). A common origin connects the properties of a word not laterally, but vertically. The various uses of "authority" do not have an essential property, one required for reference, but only perhaps a common source, a single linguistic root from which the properties spring. Roots are not an explicit part of the colorful foliage of language usage; but they can, and do, illuminate and connect the variety of uses to which we put words. The cluster of properties associated with "authority" is more understandable, more of a family even, if a common origin is unearthed, even though no essential property dutifully extends through the events described with the word.

(5) Cluster properties may be ranked in importance (Putnam, 1975). No one property may be common, but some may be more central for successful reference than others. *Bargaining*, for example: (a) mutual compromise to reach an outcome; (b) mutually beneficial returns, equal distribution of power, parity of effect, consensus on arbitration rules, and so on. The (a) property and the properties listed under (b) comprise a standard cluster (none are essential for reference). Sometimes bargains may be struck between unequals in power, effect may be skewed, or the parties may disagree on arbitration rules, and so on. Bargaining might also on occasion result in an outcome which corresponds exactly to one party's values, hence involving no compromise from that party's point of view. (The bargaining transaction might simply reveal an asymmetric possibility of agreement not clear beforehand.) If all the properties listed were missing, then the event would not be a bargaining transaction. But, as with standard clusters, we cannot pick out one property as always required for reference.

The difference between (a) and (b) is substantial, however. Property (a), "mutual compromise to reach an outcome," seems to catch the very spirit (if not the essence) of bargaining. It is a property possessed by paradigmatic occurrences of bargaining, almost a stereotypical use of "bargaining" in the language. In any "definition" of bargaining, such a property, or one very much like it, can be expected to occur. Speakers who exclude the property from their linguistic accounts of "bargaining" are obligated to offer a reason for the exclusion, or fail a rough Rorschach test for linguistic competence. The property is contingent, not essential; for "bargaining" can occur without mutual compromise. But property (a), like the property of "striped feline" for "tiger" (Putnam, 1975), seems more central to the use of "bargaining" than the properties in (b) (which are not so vital to our understanding of a bargaining transaction).

Clusters (2) through (5) modify the logic of standard cluster analysis. The primary function of the cluster thesis, at the beginning, was to deny the traditional distinction between accidental and essential properties by denying the possibility of essential properties. If correct, the standard-cluster approach discredits the efforts of those trying to define "politics" in taxonomic terms, for no property is essential for referential use of "politics." Though none of the (2)–(5) variations on "cluster" revives the Aristotelian distinction between accidental and essential properties, each provides for a surer sense of "defining" than does the standard cluster. Arrangement (2) extends necessity to the entire cluster; (3) picks out competing interpretations each with definitive status; (4) introduces linguistic origin as an illuminating connection for disparate properties; (5) separates important from unimportant properties along a contingency scale. Arrangement (5) in particular seems to open the door for *central* (though not taxonomic)specifications of political concepts. How wide the door can be opened depends on the importance ascribable to central features of concepts.

Structural Features (1):
Core Properties

In the beginning, when only a few of us existed, a famous story was accepted as gospel by true believers. The story: Language is divisible into types of statements according to a veritificationist thesis. Statements verified through experience are synthetic, or empirical, like "Bachelors are tall men." Other statements are verified by means of the definitions of words and so are analytic, like "Bachelors are unmarried men." Synthetic statements are always contingent (they could be otherwise), never necessarily true. Analytic statements are necessarily true, but make no claims on experience (they are tautologies). Accordingly, necessary truth is linguistic only, never synthetic.

Such a story, in rough outline, has been endorsed by those comprising the broad tradition of logical empiricism (Ayer, 1936; Hempel, 1965; Popper, 1959, 1962; Carnap, 1966). The story's edges, where it begins and ends, have cf late been treated roughly. Attempts have been made to attach a descriptive component to analytic statements, extending them in this way to experience (Buchvarov, 1970). "Red is darker than pink," for example: the statement is true by definition, yet also knowable by observation and seeming to assert something about experience. Synthetic statements, the other end of the story, have been given a retentive status rivaling the necessity of analytic truth (Locke, 1969). "Life is finite," "New York is geographically more than five inches from Paris," for example: the statements are synthetic, thus contingent, but impossible to falsify in the world as currently conceived.

The blurring of the edges between analytic and synthetic statements has been extended by some to a denial that any distinction can be established between analytic and synthetic (Quine, 1971). Instead of a dichotomous arrangement of statements, analytic or synthetic, language can be seen as distributed over a field whose boundary conditions are experience and whose core is resistant to change or dismissal. Any statement in the field can be maintained or dismissed if enough adjustments are made in other parts of the field. But at the core of the field are statements whose retention is required for the maintenance of whole sections of the field. ("Life is finite" and "Bachelors are unmarried men" might qualify for core status.) At the periphery of the field are statements which can be dismissed (falsified) without substantial alteration elsewhere in the field. ("The dog population is increasing in Washington, D.C." might be a candidate for peripheral status.) The varying retentive power of the field is indifferent to analytic versus synthetic distinctions, allowing each type of statement to occur anywhere in the field, and may even be said to undermine the distinction between the two types of statements in replacing "necessity" (the traditional domain of "analyticity") with the more generalized idiom of "retentive power" (my phrase).

Arranging language along a field of varying retentive power is a pragmatic program so long as any statement, at least in principle, is dismissible. But the pragmatic character of the program still allows for statements which are necessary for the maintenance of attendant propositions. If we extend the general program to political discourse, a series of concentric circles might be imposed on arrangement (5) clusters. Some properties of concepts might have strong core status in the maintaining sense: dismissal renders all or most attendant (or more peripheral) properties unintelligible. Such core properties would have a fixed status for all ordinary uses of the concept; for the absence of the core properties would make the remaining cluster of properties incapable of successful reference.

A second look at "democracy," for example: (a) popular sovereignty; (b) distribution of power, economic parity, widespread participation in politics, open access to power, dissemination of information, system capacity to respond to citizen preferences, civil liberties and rights, and so on. Property (a) is more central than the properties listed in (b) for referential use of the term "democracy." Any one, and perhaps some, of the (b) properties may be absent in a democratic system. But the absence of (a), "popular sovereignty," usually prohibits describing a system as democratic. The core status of "popular sovereignty" is sometimes recognized by viewing some portion of the (b) properties as necessary conditions for "popular sovereignty." At other times, "popular sovereignty" is held constant through a variety of political forms, e.g., unanimous direct democracy, representative democracy, each of which expresses the core property (which itself does not vary).

The fixed status of "popular sovereignty" in "democracy" can be exhibited in yet another way. Recent debates have occurred on the importance of widespread participation in democratic systems. Some have maintained that functional equivalents like "apathy," when combined with access routes to power, can satisfy democratic rule in practice (Dahl, 1961). Others have stressed the importance of actual participation for full and healthy democracies (Walker, 1966; Bachrach, 1967). None of the disputing parties has withdrawn from a conceptualization of democracy in terms of "popular sovereignty." The debate turns over the proper indicators for "popular sovereignty," what is necessary to demonstrate that a system does have popular sovereignty. Contrast such a debate with a hypothetical disagreement between Mao Tse-tung and Dahl, Walker, and

Bachrach, where precisely what might be at issue is whether a revolutionary vanguard or the people at large are the proper sovereigns in "true" democracy. *This* debate, over the core property of "democracy," is more akin to a conceptual dispute over meaning, perhaps even one of those "essentially contested" dialogues attached recently to the concept of "politics" (Connolly, 1974).

The effect of core properties on cluster analysis is settling. A core property is strongly attached to successful reference. Remove the core and, normally, the term containing the core cannot be used for reference. Remove the attendant, or peripheral, properties and reference can occur. Disagreement over use of concepts can be sorted out along the field of concentric circles. Dispute near the periphery is not "essential," while a core dispute is. Any core property may be altered. But failure to maintain the core requires a rearrangement of other parts of the field of language occupied by the concept, perhaps analogous to those conceptual shifts some see at the heart of "scientific revolutions" (Kuhn, 1962) or perhaps—more prosaically—a shift in the *meaning* of the concept.

Structural Features (2):
Essential Properties

A stronger sense of "necessity" than provided by even core properties has lately been claimed for language. The pragmatic arrangement of language in terms of retentive power separates central from peripheral properties. For example, that tigers are animals is more difficult to revise (more central to the term "tiger") than is the proposition that all tigers are stupid (Putnam, 1975). But we can imagine a world where tigers are, in some way, not animals, or at least not unambiguous animals. Imagine a process in the future which brings sculptures of tigers to life. If one of the properties of animals is being born, then it is at least open to question whether such created tigers are really animals.

Some properties, however, seem fixed in all possible worlds. Water, for example, has the chemical structure H_2O. No substance in any world can be water which is not H_2O. Gold, and only gold, has the atomic number 79. Lemons again: (a) a certain chemical structure; (b) yellow color, ovoid shape, acid taste, a certain size and hardness, skin of a certain texture, origin in sun-belt states, and so on. The properties in (b) are standard cluster-types. Each is contingently associated with "lemons," none are required for reference. But the (a) property, a certain chemical structure, is necessary. Such properties as chemical structure, atomic number, are "rigid designators": they refer to the same thing in every possible world in which the term designates (Kripke, 1972). No other item can have the designating property; and no item can be water, gold, lemons, etc., without the designating property. It is not too extreme to call such properties metaphysically necessary, required for the very existence of the items with these properties (Putnam, 1975).

Other properties are necessary in all possible worlds, but are not sufficient for successful reference. It is a contingent fact about Henry Kissinger that he is married to a woman named Nancy, a necessary fact about him that he was born of the parents he was born of. Mr. Kissinger would still be the human being he is if he had never married at all, though he would not have been Henry Kissinger, or Henry Kissinger would not have existed, had his parents not had children. Whether Kissinger still would have been Kissinger had he emigrated to England, never coming to the United States, is more difficult to say. A theory of human identity is required to sort out the complex facts which make people what they are, and even to state what it is people are when described with proper names. But while the rich complexity of human identity waits to be sorted out, those properties necessary though not sufficient for identity, e.g., "progenitor," can be isolated.

Natural-kind words like "water" and "gold" are fixed to necessary and sufficient properties. They are not cluster-terms. Nor do they contain pragmatic "core" properties. They seem rather to exhibit deep structures invariant across counterfactual conditions. Properties, like "progenitor," which are necessary though not sufficient for identity also resist both cluster and "core" analysis. Fixed structures are, however, consistent with variation in extension. What is *called* water, gold, even Henry Kissinger, may differ with time and place. But the variety of items described by structural terms does not affect the structural sense, or meaning, of the term. Water, for example, was still H_2O even when chemical structure was (not so long ago historically) unknown and liquids were mistakenly labeled as water which chemical analysis today can set apart from the genuine article (Putnam, 1975).

One more contrast. Constitutive rules are commonly distinguished from regulative rules (Searle, 1970). Constitutive rules "define" an activity while regulative rules merely regulate a practice defined in terms of other rules. The rules of chess, for example, constitute the game of chess, while the rules of etiquette regulate the pre-existing activity of eating. Constitutive rules are core properties of practices. Break the rules of chess, move the rook diagonally on purpose, say, and you are not playing chess. (Eat impolitely, however, and you are still eating.) But constitutive rules can be modified while retaining the activity. If the ruling federations of chess decide to restrict pawns to one-square movement even on first-moves, chess is still "chess" (though different). Like all core properties, revision at the center changes many other sectors of the practice (in this case, most importantly, strategy rules). But revision is possible. Fixed designators and necessary properties, however, are not revisable. Water and gold are not water and gold if the relevant structure is different; and Kissinger is just not himself with a different set of parents. Deep structures like chemical structure, atomic number, and progenitor are, finally, not pragmatic cores but fixed properties of terms very much like the classical sense of "essence."

Structural Features in "Politics"

Does "politics" have either (a) core terms, or (b) fixed structures (rigid designators or invariant necessary conditions)? The range of things describable by the word "politics" is vast and uneven. Governmental forms (democratic, authoritarian, etc.), types of policies (distributive, regulative, maximizing, ethical, etc.), types of goods (public, competitive), locus forms (governing, responses to governing), policies and laws merged with practices (assigning roles and offices, changing the rules of the game, altering the conditions of action)—the list seems endless both as to type and particular content. Precisely this heterogeneity is the bane of taxonomic "definitions" of *politics* and the strength of claims for cluster-analysis in political discourse. We have seen, however, that structural features can sometimes be found in what appear to be clusters (and no more) of properties. If structures can be located in "lemons" and "democracy," perhaps something reasonably fixed in "politics" can be discovered as well.

Core terms seem to be characterized by three conditions. (1) They are constant with variation in all, or most, other properties of the term. (2) When they change, all, or most, other properties of the term change as well. (3) A change in core terms ordinarily produces a change in the sense, or meaning, of the term. Democracy again: "Popular sovereignty" (1) is constant with retentions or deletions in attendant properties like distribution of power, economic parity, widespread participation, and so on; (2) can be changed only at the cost of making unintelligible, or radically altering, the attendant properties; (3) is dismissible only on alteration of the meaning of "democracy." On (2), for example, some properties, like economic parity, have entirely different implications in political systems with no popular sovereignty. (An economic leveling designed to escape the pernicious relationship between economic and political power pointed out by Marx and others, to insure democracy, in other words, is pointless if popular sovereignty is abandoned.) Other properties, like widespread participation (or some functional equivalent), are literally unintelligible, or paradoxical, in the absence of popular sovereignty. On (3), the dismissal of "popular sovereignty" from the concept of democracy signals a shift in meaning to some fundamentally different political theory (Mill to Mao Tse-tung, say).

Are terms similarly central to "politics" itself? Two terms seem to be excellent candidates for *core* location: (1) the sense in which politics denotes agents acting on one another, "directing" one another's behavior; and (2) the sense in which "aggregations," or collections of agents, are the acting units in politics. The first is suggested by traditional concerns for power, authority, in the history of political thought, but also describes more modern transactions like bargaining, gaming (action in conditions of no-authority), providing rational incentives (Olson, 1971), control of agendas and general social conditions. "Directiveness" also covers human behavior originating in both *decisions* and *nondecisions*, where the latter can accommodate tradition, habit, unconscious behavior in general (maps can give "directions," for example), as well as that species of politics emphasized of late (Bachrach and Baratz, 1963, 1975) where people act upon one another by *not* making decisions which could transform some state of affairs. The popular phrase, people "getting" one another to do something (de Jouvenal, 1963), seems central to "politics." The second feature reflects the group or societal sense of "politics." Political action is rarely singular, or isolated in its effects. It is *public* in its extension to, and origin in, aggregations of people.

Some contrasts may be helpful. The desert-island metaphor makes a small initial point. A single, isolated figure is not suitable for the rich transactional language of politics, like bargaining, power, authority, representation, and so on. If no one is around on the island to "get" to do things, the political terms are not referential. The more interesting use of the desert-island metaphor is its easy extension to nontransactional language. Morality, for example. The solitary figure may still be a moral creature, self-directing according to moral rules of self-betterment (Falk, 1963). Maintaining mental alertness and curiosity, exploring the desert-island fairly and honestly in terms of self-imposed methods, keeping healthy—generally avoiding the Howard Hughes-type decline in human capacities can be a reasonable moral goal. Morality, we would say, can be minimally satisfied by action towards the self, while the language of politics requires, minimally, action toward others.

Or imagine a world (like the one Adam Smith imagined) where no direction of human behavior ever takes place. Things just happen in a happy state of natural coordination (or independent movement) with no one ever "getting" anyone to do anything. Again, such a world has no referential use for the language of "politics." Such terms as "authority," the "state," "sovereignty," and so on, do not describe anything intelligible in nondirective worlds. Two more points may sharpen the contrast. First, the only "political" form advanced as nondirectional is unanimous direct democracy. Yet this decision-rule is notoriously deficient in resolving conflict, or providing incentives for action; for it fails to function exactly on those conditions which occasion politics, the need for direction to settle or manage social divisions, or to get people to secure those goals they *do* agree upon. Second, utopian proposals for nondirective worlds, like those Marx envisioned, typically see these worlds as ending "politics," once more on the quite reasonable grounds that the heart of politics is the direction of some by others.

More formal treatment of the two candidate terms, "directiveness" and "aggregation," supports their central status. (1) Each of the two terms is constant with variations in the other terms of political theory. Aristotle and Hobbes, for example, held opposed views on the proper purpose of the state, to promote the good life (Aristotle) versus to guarantee order (Hobbes). Yet both presume agents acting on one another in aggregate units. Political arrangements may shift from democratic to authoritarian, policies may regulate, allocate, and so on, without affecting the retention of the two candidate terms. (2) A change from directive to nondirective social units, or from aggregate to singular units, significantly affects all other terms in political language. Such a change renders political terms, power, authority, representation, and so on, unintelligible, or eccentric in character (like a Genet-like play about isolated "authority" figures with no one over whom to exercise control). (3) Jettisoning the two candidate terms shifts the very meaning of "politics," and perhaps even makes the concept meaningless.

A final exercise. Compare the two candidate terms, "directiveness" and "aggregation," to other terms in the language of politics. The exercise is now this: subtract the candidate term from the language, look at the results, and put it back. Then subtract a rival term, look at the results, and put it back. The question to ask in comparing the results is—are substitute conditions available in political experience for the subtracted term? If we subtract "directiveness," *all* directive terms, then we are back to the nonpolitical worlds imagined earlier. The same seems true of "aggregation." Now compare with "representation." If we subtract "representation" from the language, *all* "representation," we can still have political arrangements, for some political orders are not representative (however the term is defined). This same sense of substitutability seems also true of many other "political" terms, perhaps all. Politics can occur in the absence of many terms, but not, it seems, in the absence of the two candidate terms. The rock-bottom ideas in the concept of politics seem to start with some form of "directiveness" and some grouping or collecting together covered by the term "aggregation."

The metaphor of concentric circles seems appropriate. "Directiveness" and "aggregation" have strong retentive power at the core of political discourse. Then, moving to other circles, we find terms with less retentive power, perhaps governmental forms which express fundamental arrangements of "directiveness" and "aggregation." Further from the core may be found terms which can be retained or dismissed with diminishing effect on other sectors of the language field. Wittgenstein, developer of "family resemblance," in earlier work allows that "the river-bed of thoughts may shift." But he distinguishes between the "movement of the waters on the river-bed and the shift of the bed itself; though there is not a sharp division of the one from the other . . . and the bank of that river consists partly of hard rock, subject to no alteration or only an imperceptible one, partly of sand, which now in one place now in another gets washed away, or deposited" (1972, p. 15). The two terms, "directiveness" and "aggregation" seem as hard rock in the language of politics.

Are the *core* terms of "politics," *directiveness* and *aggregation,* also essential properties? Is the rock (in the river-bed metaphor) fixed for all possible worlds?

It does not seem so. A more modest interpretation of "essence" has been recently revived from Aristotle: essential properties are those which are necessary for the existence of a thing; the absence of the properties makes the thing go out of existence (Brody, 1975). If we limit this understanding of essence to *this* world, as we know it, then the two core terms of "politics" are essential properties: "Politics" ceases to occur when these two terms are withdrawn (though social action can occur). But the stronger sense of essence implied by rigid designators is impossible to support. Chemical structures, atomic numbers, progenitor, resist human manipulation; thus they can be invariant across changes in conventions. But "politics" is a human construct, manipulable, not fixed for all logically possible worlds. "Nominal essence" might be the right phrase (Locke, 1969): capable of basic alteration, but entrenched in the logic of political discourse as we know and use it in our world.

Some Implications of Structural Features

The recognition of core terms, or a structure in "politics," has several implications for political inquiry.

(1) The conclusions of cluster-analysis have been restricted to extension, or scope, and not carried over to all aspects of sense, or meaning. The heterogeneity of "politics" is in its extension to events. A heterogeneous extension is still consistent with a homogeneous core. What we seem to have with "politics" is a concept with a structure extendable to events unlike one another on other criteria. The reasons for the expansion or contraction of the descriptive scope of "politics" have not been explored here (though their origin in some fundamental value-set seems plausible). But core terms permit this type of variance in extension.

(2) The efforts of those attempting to define "politics" in taxonomic terms have received new life, though the life form is alien to the taxonomic tradition in important ways. Taxonomic definitions are completely satisfied only with a "defining" class property extending through all events describable as political. Such properties are only weakly realized with the recognition of core terms.

(a) A satisfactory definition of a term sets out the necessary and sufficient conditions for reference. The two core terms of *politics*, "directiveness" and "aggregation," are only necessary, not sufficient, conditions of the concept. More formally: whenever p (politics), then d, a (directiveness, aggregation); but it is not the case that whenever d, a, then p. (Tennis leagues can be directive aggregations, for example, yet not necessarily "political.") Like "progenitor," the core terms must be present for reference, but do not function to identify the event in the description. Thus they do not provide a full definition of politics.

(b) The retentive power of the structure of "politics" is in part a variable of the generality of the two core terms. "Directiveness" can take a variety of forms, for example, commands, recommendations (giving advice, or directing a person in the best way), rule-change (indirect directing, as a map "directs"), and so on. Aggregations are, logically, collections of units, unorganized wholes without particular form until the arrangements of the units are specified. So the centrality of the core terms is purchased with the loss of particular interpretation.

(c) Marginal, and deviant, uses of "politics" must be tolerated. When a doctor is called "politically" astute, when the church is said to play at "politics," when Robinson Crusoe finds Friday to give orders (and advice) to in dyadic (not aggregate) transactions, the term "politics" has referential power with only fringe benefit from the core terms.

A reasonable person conceding (a), (b), and (c), might be inclined to ask what is left of the taxonomic approach to "politics." A reasonable answer: as much as it is possible to salvage. The complex variety of political life resists class definitions. Only necessary conditions are constant in this variety, though such a structure suffers from generality and the chronic marginal use of "politics." But the core terms of "politics" provide an alternative to cluster analysis. They are similar to taxonomic definitions in one important respect: they state an invariant feature of "politics" constant across references of the term. The structure of politics as such can be studied, not the politics of this-or-that society or theorist.

The dividends to be drawn from such a study depend on the possibility of extended formal analysis of the two core terms. "Directiveness" is amenable to standard interpretive approaches, leading to the open variety of secondary terms (commands, recommendations, bargaining, etc.) which are linguistic correlates of more traditional political forms (power, authority, pluralism, etc.). A careful look at the secondary language of "politics" may provide cleaner distinctions among political arrangements than the broader terms current in political theory; e.g., a linguistic analysis of "command" may lead to richer insight than a study of the term "power." "Aggregation" is suitable for a study of composition patterns, in particular how individual units comprise wholes and whether holistic language is always reducible to, or derivable from, the language describing an individual. A typology of wholes can suggest formal patterns of social arrangements and reasoning forms (Frohock, 1975). Also, the core terms of "politics" can be extended into other conceptual areas, e.g., morality, with assurances that the conclusions reached will be more enduring than an inquiry into surface similarities and differences. Such formal analysis, with the status of a *core* inquiry, is impossible with standard clusters of properties. The centrality of the analysis, and its ubiquitous quality, is reminiscent of, if not exactly parallel to, traditional taxonomic approaches to "politics."

(3) Research-utility, as a criterion of adequacy, is itself restricted with the acceptance of core terms. If "politics" is specified in terms of convenience instead of necessity, the dominant influence in conceptualization is what students of politics are interested in studying. It is a commonplace that the history of political thought, especially since Hobbes, has been concerned with the imperative form of the state. The emphasis on imperative transactions (power, authority) is maintained even as the language of political theory has shifted from the "state" to the "political system." Such interest can be explained, at least in part, by the development of the modern state. A causal chain is thus suggested, from (a) developments in politics, to (b) research interests, to (c) definitions of "politics." A *core* account of politics severs the connection between (b) and (c): just because students of politics are interested in a political phenomenon (like the modern state) does not constitute sufficient conditions for defining "politics" in terms of the models (imperative, or command, transactions) which express such interests. Core terms express structures reasonably fixed across changing research interests. One helpful consequence of this fixed quality is that current fads, no matter how fashionable or historically interesting, are not confused with the more enduring content, and important themes, of "politics."

Thus, a *core* account of "politics" is also self-consciously opposed to historicism, especially as it leads to the view that "politics" is whatever philosophers at various historical times have thought about the term (Sabine, 1939). Historical variability, on a *core* account, can only apply to the peripheral properties of "politics."

(4) The essential contestability of "politics" is qualified. The pragmatic nature of core terms makes them, along with all other terms, contestable. But, (a) contests over core terms are unlike contests over peripheral terms. A "core" dispute is over the skeletal form, or minimum conditions, of meaning, while a peripheral dispute is typically over extension. (b) Contests over the extension of "politics" do not always affect the necessary conditions of meaning. The core terms may be maintained in disputes over what counts as "politics." (c) The fixed status of core terms in the language of "politics" suggests an absence of contests over their retention. (d) The reliance on "exemplars" in the introduction of the idea of "essential contestability" (Gallie, 1955) indicates that effective contestability requires some terms to which all parties assent, a condition the core terms may happily provide.

(5) Finally, that which core terms *cannot* do may be as important as any other consideration. Core terms are necessary, not sufficient, features of "politics." Thus, they lack identifying powers in reference. It follows that distinctions between political and other variables are matters of convention, vulnerable to sociology-of-knowledge arguments. The conventional status of such distinctions is especially important in attempts to mark off political from economic variables. Since Aristotle, students of politics have been aware of the relationship of economics to politics, a relationship Marx saw as pernicious. The restriction of the structure of "politics" to nonidentifying terms indicates that where the line is drawn between the political and the economic is conventional only, disputable without substantial cost to the language field of "politics" and conceivably a variable of some basic value-set. Recent shifts away from the neoclassical view of the state to the awareness that economic units can be the ultimate directors of society is perfectly consistent with a *core* account; for the nonidentifying nature of the core terms suggests that the fixed locus of "politics" is, finally, indifferent to a politics-economics distinction. Who and what directs is a crucial issue in the contestable area of political language. That direction takes place, with aggregate agents, is the more enduring center of "politics" as a minimum concept of social action.

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Alienation and Justice in the Market*

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This article elucidates and critically evaluates the Marxian ideas of justice and alienation by distinguishing between the class structure of capitalism and the use of the market as a mode of allocation. The argument is that the social relations of production, not the market mechanism, are primarily responsible for exploitation and alienated labor under capitalism. It follows from this that both the labor theory of value and the marginal theory of productivity, as theories of the market, are consistent with the Marxian theory of exploitation. An additional implication is that market socialism, the use of market arrangements against the background of socialist productive relations, is not only compatible with, but required by, the values that define a socialist society.

"Communists preach no morality at all," say Marx and Engels (1964a, p. 267), but if the latter fail to preach morality, or lack an ethical *theory*, they steadfastly display an ethical sense or commitment which, as Ollman indicates (1971, Ch. 4), is "internal" to their explanation of capitalist reality. My aim in this article is to elucidate Marx's sense of justice in more explicitly theoretical terms and to critically evaluate the Marxian ideas of justice and alienation by distinguishing between the class structure of capitalism and the use of the market as a mode of resource allocation. My basic argument is that the social relations of production, not the market mechanism, are primarily responsible for exploitation and alienated labor under capitalism.

I begin by focusing on relevant aspects of the recent Cambridge controversies in the theory of capital. This allows me to show that neither labor nor marginal theories of the market count as theories of exploitation. Following Lange (1968), I derive the Marxian conception of exploitation from an account of the class structure of bourgeois society, a conception independent of the truth or falsity of the labor theory of value. Next, I bring out the ethical assumptions underlying Marx's theory of exploitation and show them to rest on a sense of justice comprised of compensatory and egalitarian principles. Having demonstrated that the market, as an allocative device, is perfectly compatible with these principles, I move to argue that market socialism—the use of market arrangements against the background of socialist productive relations—is required by these principles. This line of argument calls into question the consistency of Marx's sense of justice, on the one hand, and his unyielding opposition to the market, on the other, and resolves the inconsistency in favor of the market. Finally, I locate the source of Marx's antagonism to the market in his theory of alienation and deal with the claims that (a) market *efficiency* must necessarily conflict with unalienated labor, and (b) "commodity fetishism" as a form of alienation need be a result of commodity-producing labor. I maintain that the first claim is false and, incidentally, non-Marxist, and that insofar as Marx attributes alienation to the market, he fails to make good on his theory of "commodity fetishism." My article emerges, then, as both an account of Marx's sense of justice and a defense of market socialism.

The Marxian Sense of Justice and the Market System

Cambridge Controversies. In her *Essay on Marxian Economics*, Joan Robinson draws a distinction between the productivity of capital goods and the productive contribution of capitalists: "Whether we choose to say that capital is productive or that capital is necessary to make labor productive, is not a matter of much importance. What is important to say is that

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owning capital is not a productive activity" (Robinson, 1966, p. 18). Since characterized as a touchstone of the "Cambridge controversy," this elementary distinction brings together neatly the composite features of the Marxist idea of exploitation and lays bare the reasoning, not often explicitly couched by Marx and Engels, standing behind the Marxian sense of distributive justice. The Cambridge controversies in the theory of capital bear on the distributive issue by focusing on the inability of neoclassical marginal theory to account for profit as the marginal product of capital. The neoclassicists have it that the rate of profit is the "equivalent" of the marginal product of capital. But this cannot be the case because, as two Cantabrigian critics put it, "it is necessary to know the rate of profit in order to aggregate capital into a single quantity to determine the effect on output of varying it by a unit amount, i.e., its marginal rate of return". (Hunt and Schwartz, 1971, p. 19). In other words, "attempts to relate the return to capital to its contribution are based on circular reasoning, since it is impossible to conceive of a quantity of capital independent of the profit rate" (Lebowitz, 1974, p. 387). Hunt and Schwartz elicit the likely conclusion that the distribution of the wage and profit shares of total income must be determined by something other than capital's marginal productivity, e.g., by the distribution of relative market power or conflict between workers and owners (1971, p. 20; also see Hunt and Sherman, 1972, p. 41). And Lebowitz, seeing the connection between the demise of the marginal explanation and the neoclassical "justification" of income distribution, delivers the upshot: "the theoretical nexus between the returns to the owner of capital and the contribution of means of production has been shattered" (1974, p. 387). After all, if it makes no conceptual sense to assert that the level of the rate of profit is calculable as the return to capital investment, then how can it be claimed that the owners of capital merit their income as a consequence of their investment?

The force of this criticism should not be underestimated, but the general point established by Robinson in the quotation cited above strikes deeper. For it suggests that even if the marginalists *could* make good on their argument, and apparently their argument holds in very special cases (cf. Nell, 1972a, p. 13), the critical point about income distribution would still stand firm. It does not follow that because capital, understood as "capital goods" (produced means of production) *or* as a "fund of value" (the value of property), is productive, that the owners of capital (defined in either sense) deserve their profit income. Additional argument is required to demonstrate that, and it is here that the neoclassicists have virtually *nothing* to say. Indeed, modern neoclassicists delight in saying nothing explicit about the issue of distributive justice, an issue they locate outside of their "scientific" purview and place instead in the realm of ethics and value judgments.[1] But if the neoclassicists refrain from dealing openly with the ethical issue, they nonetheless proceed to explain the world in such a way that cultivates an evaluative attitude, and it is precisely its implicit defense of capitalist inequality that prompts Marxists to dub neoclassical theory a species of "vulgar economy," a mode of analysis that paints a distorted picture of capitalism as the best of all possible worlds. Certainly one of the reasons that the neoclassicists want to save the idea of the marginal productivity of capital is that they believe the answer to key ethical questions turns on its truth or falsity.

The ethical issue at hand is: do capitalists deserve their profit income as a consequence of their owning capital? The answer must be "no," and not just because the marginalists' theory has been riddled by its Cantabrigian critics. Remember that marginal theory purports to *explain* the distribution of income in the capitalist market as a species of exchange according to which each productive factor contributes to production. This explanation suggests, but does not constitute, an ethical justification, so that even if the theory were true it would not follow that the owners of capital deserve their profit income. In the same way, the labor theory of value purports to *explain* distribution under capitalism according to which labor is ultimately the only productive factor; profit income is the surplus yielded by the labor factor and its size depends on the class struggle in both its economic and political dimensions. But even if the labor theory of value were true, while it suggests where an ethical argument might lie, it does not constitute one. It is logically possible for the labor theory of value to hold true and for the defenders of capitalist inequality to maintain their defenses. Someone could agree that the value of commodities is determined by socially necessary labor but go on to argue that people should be rewarded according to some politically designed distributive scheme, or according to

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[1] Paul Samuelson, for example, wishes to leave it to "the citizenry to ultimately decide moral issues" and, presumably, he hopes the "citizenry" defaults to the "expert" in the matter of "pointing out the feasible alternatives and the true costs that may be involved in different decisions" (1964, p. 8).

"luck," religious scripture, or a variety of other distributive criteria. There is a sense in which the controversies in the theory of capital and the labor theory of value, relating as they do to *explanatory* theories, cannot resolve the controversies surrounding the ethical issue of distributive justice. To repeat, the explanatory theories bear on theories of distributive justice, but the former do not contain sufficient information to imply ethical conclusions.

Labor Theory of Value Not a Theory of Exploitation. This point deserves expansion since it is mistakenly assumed by many Marxists and neoclassicists that the Marxian theory of exploitation stands on the truth of the labor theory of value. On the Marxist side: "the Marxian doctrine of surplus value is based . . . on his teaching of value. That is why it is important to keep the Marxian theory of value free from all distortion, because the *theory of exploitation* is built on it" (Leontiev, n.d., p. 89). From a non-Marxist perspective: Why do Marxists bother with the labor theory of value? Because a labor theory of value is necessary to Marx's theory of class exploitation (Gordon, 1968, p. 137). Neither of these claims can be substantiated. The classical labor theory of value states that in a competitive exchange economy, individual prices are proportionate to the socially necessary quantity of labor expended in the production of commodities. Adam Smith provided his familiar deer-beaver illustration to prove that in a simple market system the ratio at which commodities exchange must correspond to labor-time ratios.

> If among a nation of hunters, for example, it usually costs twice the labor to kill a beaver which it does to kill a deer, one beaver should naturally exchange for or be worth two deer. It is natural that what is usually the produce of two days' or two hours' labor should be worth double of what is usually the produce of one day's or one hour's labor (Smith, 1910, pp. 41–42).

If deer and beaver were to sell for the same price, the hunters would lack the incentive to continue catching beaver and would switch to hunting deer. The supply of deer would saturate the market, lowering their price until once again beaver would sell for twice the price of deer. This particular exchange-ratio, one corresponding to labor-time ratios, would signal the point of equilibrium, since neither beaver nor deer hunters would any longer have an incentive to shift from one type of hunting to the other.

Sweezy (1942, p. 46) notes that for Smith to get this result he must implicitly assume utility maximization and mobility, i.e., conditions guaranteeing perfect mobility so that the hunters can freely maximize their advantages. But in order for the labor theory to work, additional assumptions are required, including at least the following. First, labor must be treated as a cost; only if individuals are compensated for their labor will they be moved to expend it. Second, and more importantly, the proviso must be included that commodities *ought* to exchange at prices proportional to labor-time embodied in them. One beaver exchanges for two deer not because it is "natural," but because the hunters are operating on the belief that they *should* be compensated in proportion to labor expended. Exchange is a matter of social convention incorporating a complex corpus of beliefs about rights, obligations, fairness, etc., and certain distributive principles must therefore be included as a presupposition of the labor theory of value. This does not make the labor theory a conception of the "just price," as opposed to a theory of markets, as claimed by Joan Robinson (1962, p. 29; 1973; p. 20), but it does mean that the theory can work only if the economic actors whose behavior the theory seeks to explain adopt the distributive maxim "to each according to labor expended." The theory, then, does not propose any first-order normative recommendation (the theorist need not *advocate* any distributive principles), but it does assume that those whose activity is up for explanation do adopt distributive principles of the specified kind. It is the task of a theory of justice, not the labor theory of value or the marginal theory of productivity, to assess these operative distributive principles morally. Recognizing this distinction between normative and explanatory theories, Oskar Lange emphasizes the point that "the fact of exploitation can also be deduced without the help of the labor theory of value" (1968, p. 77). He bares the exploitative character of capitalist institutions while adhering simultaneously to the marginal theory of productivity.

> If interest is explained by the marginal productivity of capital, it is only because the workers do not own the capital they work with that interest is the personal income of a separate class of people. If interest is regarded as due to a higher valuation of present than future goods it is only because workers do not possess the subsistence fund enabling them to wait until the commodities they produce are ready that the capitalist advancing it to the workers gets the interest as his personal income. Just as in Marx's case it is because the workers do not possess the means of production that the surplus value is pocketed by the capitalist (Lange, 1968, p. 78).

Exploitation Results from Class Structure. As theories of the market, both the labor and marginal accounts are consistent with the Marxian theory of exploitation. This is so because what makes capitalism exploitative is not the market mechanism per se, but the unique class features of its social environment, i.e., the "institutional datum" that separates labor from capital, subordinates labor to capital, and locates economic and political power in the hands of the owners of the means of production.

Marx distinguishes between simple commodity production and commodity production under capitalism. In the former, production and allocation of commodities are regulated by the market, but since each producer possesses and works with his/her own means of production, labor power is not itself a market commodity. Exchange-values and the prices which approximate them are determined by socially necessary labor and competition between buyers and sellers, and these prices determine how individuals allocate their work among various kinds of production. The market is self-regulating in that competitive prices select the goods to be produced and allocate resources to their production in such a way that there would be no way to improve upon the resulting economic configuration (for elaboration, see Mandel, 1968, Ch. 2; Sweezy, 1942, pp. 45–47; O'Connor, 1976, pp. 60–63).

The central identifying feature of a *capitalist* market system is the separation of labor and capital and the coincident subordination of workers to capitalists (cf. Marx, 1906, Vol. 3, p. 1025; Dobb, 1947, p. 7). Besides possessing a quantitative dimension, capital is an historically specific social relation in which ownership confers and solidifies domination of owners over workers who have been deprived of free access to the means of making a living. In a capitalist market system, where labor-power assumes the form of an alienable commodity which workers are compelled to sell to capitalists, the market functions to facilitate exploitation. It does so by automatically and continuously transferring from wage workers to capitalists the surplus created by productive labor. Given the separation of labor and capital, the market provides the means whereby capitalists are able to take systematic advantage of workers. It is not, however, the market per se that involves exploitation, but rather the use of the market against the background of the class relations peculiar to capitalism. It is the *capitalist* market that entails the exploitation and domination of workers by capitalists.

What I am getting at can be clarified by distinguishing between the allocative and distributive aspects of market prices: "The price of a commodity or factor of production is a determinant both of the use which will be made of that commodity or factor of production and of the real income which the owner of the commodity or factor of production will receive as a result of its sale" (Meade, 1965, p. 11). With respect to efficiency, Meade argues that it is necessary to attach prices to the various factors of production and to final goods and services in order to allocate resources effectively, i.e., if high prices are levied for scarce goods and low prices for abundant goods, the users of these goods will be informed automatically as to how to satisfy their wants most efficiently. The argument here, familiar enough, is that the use of competitive prices will accomplish a Pareto-optimum allocation of economic resources. But, Meade goes on, prices used for this efficiency purpose may result in an extremely undesirable distribution of wealth and income. This he refers to as the distributional aspect of the price mechanism and cites the case of international trade relations to show that a reliance on competitive prices results in a grossly unequal distribution of benefits. Meade's point is that an efficient capitalist market may, and often does, lead to an undesirable distribution of wealth and power. As Vickrey points up in his consideration of the idea of competitive equilibrium, this potential conflict between efficiency and distribution should come as no surprise since the theorem of optimal competition "takes no account of the distribution of income and an allocation of resources would be considered to satisfy this optimum property even though half of the population were starving while the other half were enjoying great luxury" (1964, p. 214; see also Vickrey, 1973). Following Tawney (1920, Ch. 5), we can refer to capitalist control and revenue as the price workers must pay as a condition of gaining access to the means of making a living. This is the core of the distributional aspect of the price mechanism in the capitalist market system.

It should be clear by now that the distribution of wealth and income in capitalist society proceeds according to the class relations of the capitalist market system and not the market itself. It is quite possible to abstract the market (as a method of allocating resources) from its capitalist context (as a method of distributing wealth and power). To paraphrase Paul Sweezy (1942, p. 56), the market does not imply capitalism. This is evident in the case of the simple market and, as we shall see, is also true in the case of market socialism. The market as an allocative device can be put to work against the backdrop of dissimilar institutional forms; its distributive operation is a function of the specific class structure of its social environment.

Summing up the argument thus far: labor and marginal theories purport to *explain* how goods get allocated and rewards distributed; the theory of exploitation seeks to *evaluate* the fairness of the distribution so explained, and it does so by focusing on the class structure of capitalism. Lange derives the Marxian conception of exploitation by "contrasting the personal distribution of income in a capitalist economy (irrespective of whether monopolistic or competitive) with that in an "einfache Warenproduktion' [simple exchange economy] in which the worker owns his own means of production" (1968, p. 78). Exploitation is the consequence not of the market but of the differences in class structure between these two systems. C. B. Macpherson provides a similar account of the nature of exploitation in a possessive (capitalist) market society:

> When land and capital are all owned by one set of people, there is a permanent change in the distribution of the whole product between persons, to the disadvantage of the persons without land or capital. Since the latter cannot resort to independent production, they cannot demand in wages an amount equal to what would be the product of their labor on land or capital of their own. Those who have the capital and land, therefore, by employing the labor of others, get a net transfer of the powers of others (or some of the product of these powers) to themselves (1962, p. 56).

Compensatory Principles of Socialist Justice. It might be objected that this "net transfer" does not entail exploitation. The profit transferred to capitalists by workers could be interpreted as payment for services rendered. If it could be demonstrated that capitalists merit or deserve their income and power by virtue of their contribution to production, and if justice requires giving each his/her due, then the charge that capitalists exploit (take systematic advantage of) workers would be difficult to sustain. The Marxian incrimination must therefore stand on the proposition that "profits as a whole are paid for nothing at all" (Hollis and Nell, 1975, p. 18). As Sherman puts it: "What we mean by exploitation is that, under capitalism, capitalists own resources but put forth no effort, yet receive a large share of the national income" (1972, p. 58). It's true enough that capital *goods* contribute to production, that workers need access to them, and that machines typically increase worker productivity, but it's not at all clear that capital *as private property* contributes to anything except the pocketbooks of the owners of capital goods. This is the essence of Robinson's distinction between the productivity of capital and the productivity of capitalists. Profit is yielded by workers and equipment produced by workers, but it is appropriated by capitalists simply because they own the means of production, and "owning capital is not a productive activity."

This conception of exploitation implies basic normative principles of a compensatory sort: in a society defined by moderate scarcity, the distribution of income should proceed according to one's contribution to production, or more generally, the acquisition of rewards should be contingent upon the discharge of social obligation. This principle is implicit in Sherman's definition of exploitation as it is in Edward Nell's treatment of the concept. Nell criticizes the well-known introductory text diagram used by Samuelson and other neoclassicists to explain exchange under capitalism:

> The flow of profit income is not an exchange in any sense. The Samuelson diagram is fundamentally misleading: there is no 'flow' from 'household supply' to the factor market for capital. The *only* flow is the flow of profit income in the other direction. And this, of course, leads straight to that hoary but substantial claim that the payment of wages is not an exchange either, or at any rate, not a fair one. For Wages plus Profits adds up to the Net Income Product; yet Profits are not paid for anything, while wages are paid for work. Hence the work of labor (using the tools, equipment, etc., replacement and depreciation of which is already counted in) has produced the entire product. *Is Labor not therefore exploited?* Does it not deserve the whole product? (1972b, pp. 46–47)

Note that Nell's conception of desert implies the acceptance of compensatory principles of distributive justice. The same is true for Marx and Engels, who speak of capitalists "extorting" a surplus from workers, "pumping booty out of the laborer," and living in idleness off the unpaid labor of the working class (Marx, 1906, Vol. 1, p. 653; Engels, 1967, pp. 98–100, 113–16). Put simply, Marx and Engels argue that, given the separation of labor and capital, workers are compelled to work for capitalists and, finding themselves in this subordinate position, must surrender to capitalists (1) their surplus product and (2) control over their work lives. This is the dual price workers must pay in order to gain access to the means of production.[2] Do capitalists *do* anything productive to merit this payment? No. As far as productive contribution is concerned, capitalists constitute a "superfluous" class of coupon-clippers that has long ceased to perform a useful function. It is people in their capacity as managers or paid employees who supervise the workplace and carry on the business of investment, leaving people *qua* capitalists in the position of "pretending to earn" (Engels, 1967, p. 115; also see Marx, 1906, Vol. 3, pp. 449–59). The only labor performed by capitalists is the "labor of exploitation" (Marx, 1906, Vol. 3, p. 450). And capitalists are joined by managers in this "labor" to the extent that the latter have as their task the *control* of workers, a function that "arises out of the specific form of capitalist production" and that will disappear after economic organization has stripped off its capitalist integument (Marx, 1971, Vol. 3, pp. 496–97). "The best demonstration of this are the cooperative factories built by the workers themselves. They are proof that the capitalist as functionary of production has become just as superfluous to the workers as the landlord appears to the capitalist with regard to bourgeois production" (Marx, 1971, Vol. 3, p. 497).

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[2] In contrast to precapitalist societies, this payment proceeds automatically, "invisibly," and without deliberation, since the nature of capitalist production

Marx moves to dispose of the other defenses erected by apologists of capitalist distribution. The justification of private profit as a payment for 'abstinence' lacks any evidential backing, there existing no discoverable connection between alleged abstinence and profit making. Marx "had only to contrast the profit and the 'abstinence' of a Rothschild to feel that the so-called 'explanation' required no further refutation" (Dobb, 1960, p. 137). If anyone undergoes abstinence from consumption in order to generate capital accumulation, it is the enforced abstinence of the working class. The idea that capitalists take risks can be handled with similar facility. First, there is a paucity of evidence that capitalists take significant risks, especially in developed capitalist countries where the highest rates of profit are realized in the most stable and secure (oligopolistic) sectors of the economy (Sherman, 1972, Ch. 8). If we operationalize a definition of risk-taking, measuring risk in terms of rates of success and failure over time, we find little evidence pointing to the downward mobility of capitalists. The structured inequalities of capitalist society make extensive movement on the stratification scale highly unlikely (cf. Mayer and Buckley, 1970, Ch. 8). Second, if the risk-taking argument is used to justify profit income, simple equity requires that the principle be impartially applied. What about risks undertaken by workers as measured by deaths and injuries on the job, unemployment, occupational disease, etc.?[3] Rewarding the supposed risks of capitalists and not the real risks of workers is a very peculiar sort of fairness, "yet according to the political economy of the capitalist class, that is the very pink of fairness" (Engels, 1967, p. 99).

Having demonstrated the indolent and parasitic character of the capitalist class, Engels concludes that "the capitalist can no longer lay claim to his profits" (1967, p. 116). He writes:

> What is morally fair, what is even fair in law, may be far from socially fair. Social fairness or unfairness is decided by one science alone—the science which deals with the material facts of production and exchange, the science of political economy.... According to what we may call common fairness, the wages of labor ought to consist in the produce of his labor. But that would not be fair according to political economy.... And thus the end of this uncommonly "fair" race of competition is that the produce of the labor of those who work, gets unavoidably accumulated in the hands of those that do not work, and becomes in their hands the most powerful means to enslave the very men who produced it.... The fairness of political economy, such as it truly lays down the laws which rule actual society, that fairness is all on one side—on that of Capital (Engels, 1967, pp. 98–100).

Engels here employs compensatory principles of justice, included in the conception "moral or common fairness," to evaluate negatively the operative distributive norms ("social fairness") of capitalist political economy.[4] Dur-

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tends to obscure the division of the working day into necessary and surplus labor time.

[3] According to James Weinstein (1968, p. 40), from 1880 to 1900 an estimated 700,000 workers were killed and 10,720,000 injured in U.S. industry. According to HEW (1973), some 14,000 workers died in industrial accidents in 1968, 90,000 suffered permanent impairment, 2,100,000 suffered total but temporary disability, and an untold number contracted an assortment of occupational diseases ranging from heart and lung disorders to deafness and hypertension.

[4] These passages assist in clarifying Marx's treatment of "fair distribution" in *Critique of the Gotha Programme.* Marx rejects the "fair distribution" (the social fairness of capitalist political economy) of bourgeois apologists as so much "verbal rubbish," and though he is understandably impatient with utopians who propose principles that have no chance of implementation at given stages of economic and cultural development, he does not reject the idea of justice or its compensatory component (for support of this interpretation, see Van De Veer, 1973). The claim is mistaken that Marx provides a purely descriptive or sociological treatment of justice, that he limits himself

ing the transition period from capitalism to communism these principles come into their own. Marx and Engels see socialism as a cooperative social union in which each individual (who is able) is expected to do his/her share of work, and in which, as Engels puts it, "no individual can throw on the shoulders of others his share in productive labor" (Engels, 1947, p. 438). The distribution of rewards is contingent upon the execution of social obligation.[5]

The classic statement on distribution under socialism appears in the *Critique of the Gotha Programme*, where Marx posits the compensatory principle: to each according to the duration or intensity of his work, from each according to his ability (1955, Vol. 2, pp. 21–25). Marx advocates this principle even while regarding it as "defective." The principle is defective when compared with the distributive maxim under communism, the imperfection stemming from the limited, slightly structured, inequalities that result from its implementation. Only under communism, where there are more goods available than there are claims for these goods, can individuals equally satisfy their unequal needs. Though defective from the vantage point of communism, compensatory principles find an appropriate place under socialism, a society defined by moderate scarcity. These principles are *compensatory* in that they justify limited inequalities by awarding larger incomes to workers who undergo disproportionate disutility in the course of working for the common advantage, though these inequalities require containment at the point where they tend to foster the development of a class-divided society. Only if workers who bear greater burdens receive greater compensation can other workers avoid the charge that they are taking advantage of their harder-working comrades.[6] The use of material incentives governed by compensatory principles of distributive justice is both consistent with socialist morality and necessary to evince the willing cooperation and maximum productive effort of everyone participating in socialist society.

Lange's postulation of a scheme of distribution comes close to the theory of justice just sketched. Assuming that the same demand price offered by different consumers must represent an equal urgency of need, he calls for a distribution of equal incomes. At the same time, "the distribution has to lead to such apportionment of the services of labor between the different occupations as to make the differences of the value of the marginal product of labor in the various occupations equal to the differences in the marginal disutility involved in their pursuit." This requires a differentiation of incomes since "to secure the apportionment of labor services required, differences in the marginal disutility of the various occupations have to be compensated by differences in income." There is no contradiction here.

> By putting leisure, safety, agreeableness of work, etc., into the utility scales of the individuals, the disutility of any occupation can be represented as an opportunity cost. The choice of an occupation offering a lower money income, but also a lower disutility, may be interpreted as the purchase of leisure, safety, agreeableness of work, etc., at a price equal to the difference between the money income earned in that particular occupation and others. Thus the differences of incomes required by condition 2 are only apparent. They represent prices paid by the individuals for different conditions of work. Instead of attaching different money incomes to the various occupations the administration of a socialist economy might pay all citizens the same money income and charge a price for the pursuit of each occupa-

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to explicating the content of the norms of distribution mirrored by different modes of production. On this account, "the Marxian critique of justice may be viewed as an attempt to clarify the role of the concept of justice in social life and to prevent its ideological abuse" (Wood, 1972, p. 245), but Marx is not understood as issuing any first-order moral judgment of capitalism as unjust. This relativistic reading of Marx has it that capitalism *is* just because "it accords with the juridical or moral rules and practices which govern distribution" in that particular mode of production (Wood, 1972, p. 268). Against this interpretation, I maintain that Marx's characterization of capitalism as exploitative implies that he assumes a theory of justice. To call something exploitative is to take a position toward it; it is a clear case of taking issue with the distributive norms of bourgeois political economy and rendering a negative assessment of them. Marx is saying that bourgeois 'fairness' is not fair. Hannah Pitkin refers to such terms as "fairness," "legitimacy," "obligation," "authority," etc., as performing performative functions. To say that something is fair is to adopt a position toward it, not to say merely that something is commonly considered fair by other individuals employing the term. See Pitkin's critique of Weber's use of the term "legitimacy" (Pitkin, 1972, pp. 280–86).

[5] When Tawney said (1953, p. 39) that "the last of the Schoolmen was Karl Marx" he had in mind the medieval concern that tied social reward to the performance of social function.

[6] Of course, nothing prevents workers who undergo greater disutility from voluntarily contributing their surplus labor. Altruism is a noble practice and is encouraged under socialism, but it is not enforced. On the distinction between justice and altruism, see J. O. Urmson (1958).

> tion. It becomes obvious not only that there is no contradiction between both conditions, but that condition 2 is necessary to satisfy condition "one" (Lange, 1938, pp. 101–02).

By way of concluding this discussion of compensatory principles, I should bring out two difficulties with Marx's account of distribution during the transition period. First, contrary to Marx's characterization in *Critique of the Gotha Programme*, there is nothing peculiarly "bourgeois" about a conception of distributive justice based on proportionate equality. Second, rewarding workers solely according to "quantity and intensity" of labor is an insufficient criterion because it ignores the varying degrees of disutility attached to different kinds of labor. Some work, though it takes considerable time and effort, may confer ample enjoyment, providing its own reward, in contrast to less satisfying work of equal duration and intensity that needs to be redressed by additional income.

The bourgeois principle of distribution is "to each according to what he and the instruments he owns produces" (Friedman, 1962, pp. 161–62). The principle assumes the institution of capitalist property and sets no limits on the extent of permissible differentials; i.e., it establishes the right to unrestricted acquisition of wealth and economic power. Because it is precisely this type of property and mode of distribution that is *absent* from a socialist society, one ponders why Marx, who possessed such a clear understanding of the historically specific features of bourgeois society, described the proportional distribution characteristic of the first phase of socialism as uniquely *bourgeois.* The idea of distributive justice that gives unequal rewards to unequal degrees of merit, in exact proportion to the inequality of the degrees, is at least as old as Aristotle and also found its way into the moral philosophy of the medieval canonists (e.g., their commutative theory of the "just price"), few of whom could be plausibly described as bourgeois thinkers. Marx is therefore mistaken in his characterization of the norm of distribution during the period of transition as necessarily or distinctly bourgeois. It can, instead, be suitably *socialist* and constitutes a perfectly reasonable principle of distribution in a circumstance defined by moderate scarcity. It is *not* bourgeois because, at the same time it issues proportionate rewards, it assumes social ownership and it strictly circumscribes the range of tolerable inequalities. Thus the principle is rigorously egalitarian.

The Market and Socialist Justice. Is the market compatible with the egalitarian and compensatory principles of socialist justice? It is, if it operates against the background of socialist institutions. Recall that the great inequalities of capitalist society stem from its *class structure.* Exploitation is not the consequence of the market per se, but of the use of the market in capitalist society, i.e., a society divided into classes of owners and workers. If the market mechanism were put to work in a society where the means of production were publicly owned and where workers controlled the polity and the enterprise, the use of the market need not interfere with the realization of egalitarian norms of distribution. As already noted, there is no logical connection between the operation of free markets and the capitalist ownership of productive facilities. In market socialism, labor-managed enterprises functioning under market conditions are neither the property of capitalists nor of the workers who utilize them; they belong to society. The right to control the enterprises, vested in their workers, stops short of the license to hold or sell shares in them; the purchase and sale of equity shares is nonexistent in market socialism. It is true that the categories of profit, rent, and interest exist in market socialism, but these serve solely as accounting prices or efficiency indicators, providing for a meaningful calculation or rational disposition of resources by indicating their relative scarcity as well as the relative preference of consumers for different products. For example, interest might be charged by banks on loans scheduled for investment by enterprises, but the price assessed on loans would not take the form of an income paid to private owners. The same applies to profit and rent. Profit serves as an evaluator of enterprise efficiency and signals the direction in which future investment will be allocated, but profit is not distributed to shareholders and will affect the distribution of income only if it meets the requirements of egalitarian income policies. The state under socialism, by enacting a variety of redistributive policies stipulated by egalitarian values, prevents a situation from developing in which income differentials exceed an acceptable point. Centrally planned taxation schemes and social welfare provisions operate to institutionalize egalitarian norms of distribution.

This means that the use of market devices does not necessitate material incentives dictated by bourgeois values. To put it very simply, if workers in a socialist society agree to implement egalitarian principles of distributive justice,[7] if politics are in command, then whatever inegalitarian tendencies inherent in an unmitigated market distribution will be offset by the redistributional activity of the state. The more efficient workers, including those grouped within the same enterprises, after the redistribution policies have taken their toll, will not be much better off than their less efficient counterparts. Profit making under such an arrangement cannot function as a *motivating* force of production precisely because all workers agree that no individual or group should expect extraordinary benefits as a result of their greater market performance. It is nonetheless true that the idea of profit making looms large in market socialism, but only insofar as it informs society where an efficient allocation of resources lies. In market socialism profit does not constitute an incentive to work but serves only as a measure of efficiency. Horvat casts some light on this point in his discussion of the price of labor. He distinguishes between gross and net wages of labor, the gross wage reflecting the marginal productivity of labor and the net wage determined by a political decision consistent with social expectations as to what wage-scale constitutes fairness. "The difference between the gross wages and the net wages represents rent which is taxed away by the Planning Authority. One may say that net wage-rates represent the supply price of labor, while the gross wage-rates are its demand price" (1964, p. 130). In the interest of efficiency, it is important that workers seek to maximize the gross wage even though they receive only the politically determined net or fair wage. In this way, justice holds sway in the sphere of distribution while efficiency reigns in the sphere of production. In market socialism the market is used to allocate resources but not to distribute wealth and power.[8] Accordingly, the use of the market is perfectly compatible with the egalitarian and compensatory principles of socialist justice. Of course, material incentives of a bourgeois sort may be used in combination with market arrangements, just as they have been used in command-type economies, but it should be clear that the decision to rely on such incentives depends on factors other than methods chosen for purposes of allocation.

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[7] In the *model* of market socialism, workers would agree. I am not concerned here with the *historical*

The Market Required by Socialist Justice. It is one thing to argue that the market is compatible with socialism, another that socialist justice *requires* the market. My aim is to defend the stronger claim, and I want to raise some difficult questions about this issue as it relates to the transition to communist society.

Communism defines a society in which the fragmentation of labor has been largely eliminated, individuals are able and willing to realize their unique capacities in the productive process, and the distribution of goods proceeds according to the maxim: to each according to his/her needs, from each according to his/her abilities. Under communism, consumption goods are produced by socially owned enterprises and distributed equally in response to individual need. Equal need-satisfaction, of course, entails an unequal distribution of goods since people presumably have different needs and wants of varying intensity. As Mandel puts it: "It is the full development of the *inequality* among men, of the inequality of their aspirations and potentialities, the inequality of their personalities, that emerges as the aim of socialism [communism] (1968, Vol. 2, p. 673). Unequal needs can find equal satisfaction because, under communism, there are more goods available than there are claims for these goods, i.e., communism is defined by a circumstance of superabundance. Since there is a complete absence of money, prices, and wages, goods are made available "free for the taking," which is to say that "payments" made to individuals bear no connection to what individuals have given in exchange. *All* consumer goods get publicly provided, neither rationed nor purchased by individual wages in a market situation. Mandel tries to clarify what is involved

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problem of getting workers to substitute egalitarian for bourgeois ideas of justice.

[8] To argue that the market be employed to allocate resources efficiently while at the same time society utilizes a socialist norm of distribution is in no way at odds with Marx's insistence that production and distribution are two sides of a single economic process in which production assumes the dominant place. The only reason that an egalitarian distribution is made possible in market socialism is that the means of production are publicly owned and secure under the control of the working class. Marx argues that "to treat production apart from the distribution which is comprised in it, is plainly an idle abstraction," and he derides the "insipidity of the economists who treat production as an eternal truth and banish history to the domain of distribution" (1904, p. 286). For Marx, "distribution is itself a product of production ... since the definite manner of participation in production determines ... the form under which participation in distribution takes place" (1904, p. 284). Marx's criticism was prompted by J. S. Mill's abstraction of the laws of distribution from the relations of production. Mill expresses the idea that "the laws and conditions of the production of wealth partake of the character of physical truths," whereas the distribution of wealth is "a matter of human institution only" (1864, pp. 257–58).

here by distinguishing between individual and social wages, noting that the social wage "foreshadows, at least potentially, the mode of distribution of the future, that is, of an economy directed towards satisfying the needs of all individuals" (1968, Vol. 2, p. 657). Already, in both capitalist and socialist societies, there has appeared the social wage or dividend. Instead of individually purchasing education, medical care, municipal transportation, street lighting, etc., these goods are provided publicly, i.e., individuals pay for these goods collectively through taxes and receive them "gratis" as a social wage or dividend. "The social wage is thus the *socialization of the cost* of satisfying a certain number of needs for all citizens" (Mandel, 1968, Vol. 2, p. 657). Under communism *all* costs would be socialized, or as present-day "welfare economists" would put it, all goods would become "public." This would not square with socialist principles of distributive justice which call for compensatory payment, but no matter, because communist society is *beyond* justice.[9]

If socialism is a *transitional* interval, then compensatory principles of justice must gradually give way to communist principles of distribution. Individuals in a socialist society must consent to the substitution of the collective provision of goods for their individual purchase in a market. Sherman points out that this substitution would be piecemeal. Prices of basic necessities would be lowered slowly while the government notes the reaction of demand to price changes. If demand is fairly inelastic, i.e., if demand fails to shoot up as prices are reduced, prices can eventually be lowered to zero. The lowering of prices and their eventual disappearance would be paid for by lowering the money wages paid to individuals in exchange for their work, by substituting a social wage for an individual wage. For example, instead of a worker receiving $20 daily, he/she might receive $18, but his/her bread, salt and fish would be "free" for the taking at consumer goods outlets. Sherman is aware of the difficulties involved in this substitution: "Obviously, in the continuing expansion of the free public goods sector, each marginal choice is a vital and controversial social decision. It would be imperative to make it as democratically as possible" (1972, p. 342).

Just what is the nature of this difficulty? Simply that the move to communism entails not the mere bypassing of socialist distributive principles but their *violation* under circumstances (moderate scarcity) that call for their continued application. Compensatory principles require rewards proportional to the duration, intensity, difficulty, unenjoyability, hazardous nature, etc., of different jobs. But the extension of the public goods sector impinges on socialist justice because it upsets this system of proportional rewards designed to govern distribution during the transition period. The increased provision of collective goods in a situation of moderate scarcity compels some individuals to subsidize the unwanted benefits desired by others while it necessarily subtracts from the range of want-satisfaction available to the former. And to the extent that compensatory principles establish just incentives to work, their attenuation may bring about a slackening in the productive effort to create the material preconditions of communist society. Thus we are faced with a clear-cut opposition between socialist and communist principles, and one that may affect the very likelihood of attaining the goal of communism.

At the minimum, socialist justice prescribes a market in consumption goods. It might be argued as well that socialist justice requires a market in production goods since consumer preferences would of necessity play a large part in determining the direction of production. Further, if individuals are to be compensated for productive contribution as fully as possible, and if the market possesses the advantage of efficiency, the case can be made that the market is desirable as an allocative mechanism because it best satisfies the demand that individuals be compensated at a maximum level. But, says Sherman, "a socialist economy, in which payment for consumer necessities is required, reinforces and produces competitiveness in people every day" (1972, p. 343). This is because, according to Mandel, "the continued existence of money and commodity economy in itself implies the survival of the phenomenon of *universal 'mercenariness' of life*. . ." (1968, Vol. 2, p. 655). Assuming the truth of these claims for the moment (I want to question them later), one may justify the infraction of socialist justice by appealing to moral principles of a different order. Justice, after all, is not the sole value and must find accommodation with other, perhaps more important, moral imperatives. Crucial to the moral vision of socialists, built into their picture of the good society, is the elimination of competitiveness, the promotion of social cooperation, and the disappearance of alienation. These aims, ranking first on the scale of socialist values, of necessity override the precedence of justice; only if justice is mitigated can communism be attained, and only under communism can the good life be actualized. Socialist justice may require the market, but more important communist values call for its gradual supercession.

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[9] As the ingenious Rawls writes: a society of superabundance "in which all can achieve their complete good, or in which there are no conflicting demands and the wants of all fit together without coercion into a harmonious plan of activity, is a society in a certain sense beyond justice. It has eliminated the occasions when the appeal to the principles of right and justice is necessary" (1971, p. 281). Nicholas Rescher writes: "the workings of the concept of distributive justice are a function of scarcity. . . . In an economy of superabundance where everyone has all that he needs and wants, the question of distributive justice no longer arises" (1966, p. 107).

Fine, as far as it goes, but we should be aware of the profound difficulties, logical and historical, complicating the move from justice to communism. There is the logical or theoretical problem of reconciling the gradual implementation of communist principles during the transition period with the compensatory type of justice advocated for this period. And there is the historical or motivational problem, for if, as Sherman and Mandel assert, competitiveness and "mercenariness" continue to exist in socialist society, what would prompt its members to execute a program gradually abolishing the commodity production to which both writers attribute these lingering evils? Each decision to substitute public for private goods would test the commitment of socialist citizens to build a communist society and in the course of that construction to surrender the practice of compensating workers in accord with labor performed, and this in a circumstance defined by a moderate scarcity that fosters competitiveness and acquisitiveness! Mandel believes that "it is necessary *first* to see the withering away of money economy through the production of an abundance of goods and services before the psychological and cultural revolution can fully manifest itself, and a new socialist consciousness bloom in place of the egoist mentality of the 'old Adam'" (1968, Vol. 2, p. 655). But surely Mandel has put things in reverse order. The phasing out of commodity production is not an automatic process, but the result of an undetermined conscious strategy deployed by socialist citizens to overcome the survivals of the capitalist past and to build a new society in conformity with communist ideals. Money and the market are eliminated gradually through a series of "vital and controversial decisions" to extend the public sector, decisions that cannot be the mechanical reflex of a money-market society since, if determinism held, these decisions would be precluded altogether. Without communist ideals occupying a dominant place in the transition to the good society, the step-by-step substitution of public for private goods could very well exacerbate the spirit of egoism and envy. Remember that such substitution effects a redistribution of scarce goods while violating compensatory principles of social justice. Whether there is a market in goods *or* goods are publicly provided, their distribution proceeds under circumstances of scarcity that lay the basis for a competitive scramble. In *this* sense, collective provision holds no advantage over market distribution during the transition period.

Why then the Marxian emphasis on dismantling the market in favor of collective provision and planned allocation? The answer lies in Marx's theory that alienated labor and the market system are inextricably connected. The theme is pervasive in Marx that under communism purposive social control or "conscious regulation in accordance with a settled plan" replaces the autonomy of the market. The market, commodity production, the use of money, etc., create a "mystical veil" covering over social reality, personifying objects and reifying human beings, and transform labor into a *means* for satisfying needs when creative labor itself should be life's prime need. Thus Marx assumes that "the return of man himself as a social, i.e., really human being" can transpire only in a marketless world. Is this assumption warranted?

Alienation and the Market

Pursuing a theme introduced in the preceding discussion of justice and the market, I want to suggest that it is the class structure of production, not the market mechanism, which is primarily responsible for alienated labor. This suggestion controverts a contention based on a popular distortion of Marx's theory—the idea that market *efficiency* must necessarily conflict with unalienated labor; it also questions Marx's hypothesis that commodity-producing labor must be alienated labor, i.e., the theory of "commodity fetishism."

Market Efficiency and Alienated Labor. Frank Roosevelt (1969) has argued forcefully that market socialism is a bourgeois idea because it sacrifices work to consumption, i.e., subordinates the satisfaction derivable from work to the efficient production of commodities scheduled for consumption. The market cuts the tie between production and consumption and transforms work into an instrument geared to the maximization of consumptive utility. The benefits of work are sought outside of work. Labor is "forced," says Marx, and "therefore not the satisfaction of a need; it is merely a *means* to satisfy needs external to it" (1964b, p. 111). Marx understands work or creative activity as "life's principal need," the gratification of which makes it possible for people to express their personalities, objectify their inner needs and talents—in short, to realize their human nature. Further, one of Marx's most penetrating criticisms of the capitalist market centers on its possessive individualism and antagonism to community life. Marx uses the concept "capitalist society" not only to express his condemnation of a social system based on inherent injustice, but also to describe the structure of social order in which some attributes of the communal organization of previous societies no longer exist. In such a society, at least in its "pure" form, individuals become so isolated and egoistic that they establish contact only when they can use each other as means to particular ends. Communal bonds get supplanted by contractual associations entered into by utility-maximizing individuals. The classical liberals, for instance, made extensive use of a metaphor picturing human society as a vast trading company with each of its members a merchant. More recently, Milton Friedman thought it useful to allude to children as "consumer goods." "The freedom of individuals to use their economic resources as they want includes the freedom to use them to have children—to buy, as it were, the services of children as a particular form of consumption" (1962, p. 33).

Roosevelt's point is that the market mechanism, whether employed in a capitalist or socialist environment, is biased in favor of efficiency to the detriment of humanized work and nonalienated social relations. Not only does efficiency require an enslaving subjection to the division of labor but also it lessens the likelihood of community by accentuating the narrowly defined role of *homo economicus* and demanding a high degree of worker mobility organized along vertical lines. The very logic of the market entails the alienation of people from their work and from each other:

> Under market conditions a minority of the workers in any industry—perhaps even one enterprise—can impose its preferences on all the rest. All it takes is one firm to choose the more efficient autocratic method of organization, say by signing a Hobbesian type of social contract, and all the other firms must follow suit—or find themselves driven out of business. Thus there seems to be a built-in tendency for a market economy—whether capitalist or socialist—to promote greater efficiency at the expense of humane working conditions whenever the two goals conflict (Roosevelt, 1969, p. 18).

So goes Roosevelt's critique, which ostensibly takes as its source of inspiration the insight of Marx and the recent prognoses of social philosophers who still think it is important to emphasize the potential benefits of humanizing the work process. Among these prognosticators is Daniel Bell, whom Roosevelt claims has offered an analysis of productivity and work which resembles that of Marx (1969, p. 15). Marx and Bell, however, could not be further apart on the subject of alienation and efficiency, and it is in the radical difference between the two that we can proceed to uncover the essential weakness of Roosevelt's position.

Bell numbers among those bourgeois ideologists who have adopted an attitude of pessimism and highbrow disdain toward certain facts of "advanced industrial society." These facts are expressions of what Bell terms the three "technologics" created by modern industry: the logic of size, the logic of "metric" time, and the logic of hierarchy (1960, p. 225). Each of the three is the product of "engineering rationality" required by "the sociological fact of increased supervision which every complex enterprise demands" (1960, pp. 225, 229). Bell explains the huge size of enterprises and the centralization of employment as a consequence of the engineer's belief that large-scale production, conditioned by the development of energy resources, is technologically efficacious. He then attributes to "the inexorable logic of rationalization" the labor regimen so carefully worked out by Frederick Taylor and other theorists of "scientific management." Taylorism, fostering the specialization of labor, the detailed fragmentation of work and the collapsing of skilled labor into simple, repetitive, and easily measurable labor (abstract labor?), is in Bell's view a kind of "social physics" in the service of heightened productivity. The atomization of the work force in turn creates the need for bureaucratic organization staffed by technical coordinators and headed by management; workers whose skills have been downgraded and whose labor has been reduced to the performance of narrow and predetermined tasks *require* hierarchical direction. "Under a complex division of labor these tasks pass out of [the worker's] control, and he must rely on management to see that they are properly done. This dependence extends along the entire process of production" (Bell, 1960, p. 229).

Bell caps his thesis of the three logics of modern industry by noting their convergence "in that great achievement of industrial technology, the assembly line: the long parallel lines require huge shed space; the detailed breakdown of work imposes a set of mechanically paced and specified motions; the degree of coordination creates new technical, as well as social, hierarchies" (1960, p. 229).

Contrast this to what Braverman says about the assembly line in his Marxist analysis of the labor process under capitalism:

> The chief advantage of the industrial assembly line is the control it affords over the pace of labor, and as such it is extremely useful to owners and managers whose interests are at loggerheads with those of their workers. From a technological point of view, it is extraordinarily primitive and has little to do with "modern machine technology" (1974, p. 232).

Braverman arrives at different conclusions about the assembly line because he has not succumbed to any of the prevailing species of technological determinism. It is not the 'logic' of technology, machines, or modernity which is responsible for alienated labor, but the uses to which technology is put (or not put) and the shape it assumes in the capitalist mode of production. "Within the historical and analytical limits of capitalism, according to Marx's analysis, technology, instead of simply *producing* social relations, is *produced by* the social relations represented by capital" (Braverman, 1974, p. 20). The notion that machines and "the requirements of efficiency" are the determining factors of the organization of work under capitalism is actually a reification of the social relations peculiar to capitalism, "nothing but a *fetishism*, in Marx's sense of the term" (Braverman, 1974, p. 229). It is, of course, a convenient fetish for bourgeois thinkers because it lets capitalism off the hook, extricates it from special blame, and shifts castigation in the direction of empty abstraction.

Bell's "logics of modern industry" are in fact the logics of capitalism. The centralization of labor in large-scale production units has its historical genesis not only in the "engineer's concern with efficiency," but principally in the aim of capitalist management to enforce greater discipline over a "free" labor force. Pre-capitalist productive units, being scattered and allowing producers a considerable degree of autonomy over their work, made it difficult for the capitalist to impose effective supervision over the pace and intensity of work. Bringing workers under a single roof provided capitalists the spatial prerequisite for establishing the tightest possible reins of control over the labor process. "It was not that the new arrangement was 'modern,' or 'large,' or 'urban' which created the new situation, but rather the new social relations which now frame the production process, and the antagonism between those who carry on the process and those for whose benefit it is carried on. . ." (Braverman, 1974, pp. 68–69). Likewise, "scientific management" is not so much a logical outgrowth of industrial technology as it is an attempt to selectively apply scientific methods to the problem of controlling labor in complex capitalist enterprises. Historically, the impulse behind the superfragmentation of work was the capitalists' concern with cheapening the price of labor. The reason for the rapid spread and intensification of the division of labor was that "labor power [could be] purchased more cheaply as dissociated elements than as a capacity integrated into a single worker" (Braverman, 1974, p. 81). The reduction of labor to its simplest form also stripped laborers of their power to acquire any modicum of dominion over the entire production process, thus ensuring that management possessed a near-monopoly of information and "technique" to execute its plans of production. Taylorism assigns the functions of conception to the brains of management and the job of mindless execution to an increasingly subdivided body of workers. In this way, as Gintis points out, "capitalists [can avoid] workers gaining enough general expertise and initiative to embark on cooperative production on their own, or to challenge the hegemony of capitalists in the factory or office" (1972, p. 14; also see Bowles and Gintis, 1975, and Marglin, 1974).

There is an intimate connection, then, between the atomization of the work force and the imperative of capitalist domination. The stratification of roles into managers and managed and the fragmentation of tasks go hand in hand, with the *former* assuming the determining place. It is the class or power relations of capitalist society which underpin the technological structure of capitalist production and which come to harness and eventually block the further development of the forces of production. Presumably this idea stands behind Braverman's assertion that the assembly line is an instance of backward technology and primarily a method employed by capitalists to maintain their dominant social position. It is a form of production conpatible with the reproduction of class privilege, but hardly likely to unleash the liberating types of technology which would accompany the all-around development of productive forces. Not efficiency, but control is the watchword of capitalist production, and the former will usually lose out to the latter in the event of conflict (cf. Zimbalist, 1975).[10]

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[10] The same can probably be said about the Soviet system. Of course, to the extent that it *can* be truthfully said, the USSR is distinctly not socialist. I am struck by the applicability of the argument of the theorists of "totalitarianism"—that "totalitarian" control goes hand in hand with the fragmentation of the

Marx and Engels explicitly view the development of the forces of production under capitalism as bringing with it the *potential* to revolutionize the nature of work. Marx speaks of the tendency toward abolishing the division of labor in modern industry and recognizes that this tendency can be realized only with the revolutionary transformation of capitalist relations of production.

> By means of machinery, chemical processes and other methods, it [modern industry] is continually causing changes not only in the technical basis of production, but also in the functions of the laborer, and in the social combinations of the labor process. At the same time, it thereby also revolutionizes the division of labor within the society, and incessantly launches masses of capital and of workpeople from one branch to another. But if Modern Industry, by its very nature, therefore necessitates variation of labor, fluency of function, universal mobility of the laborer, on the one hand, in its capitalistic form, it reproduces the old division of labor with its ossified particularizations.... Modern Industry, on the other hand, through its catastrophes imposes the necessity of reorganizing, as a fundamental law of production, variation of work, consequently fitness of the laborer for varied work, consequently the greatest possible development of his varied aptitudes. It becomes a question of life and death for society to adopt the mode of production to the normal functioning of this law. Modern Industry, indeed, compels society, under penalty of death, to replace the detail worker of today, crippled by life-long repetition of one and the same trivial operation, and thus reduced to the mere fragment of a man, by the fully developed individual, fit for a variety of labors, ready to face any change of production, and to whom the different social functions he performs, are but so many modes of giving free scope to his own natural and acquired powers (Marx, 1906, Vol. 1, pp. 532–34).

Engels comments on these passages in *Anti-Duhring:*

> Once more, only the abolition of the capitalist character of modern industry can bring us out of this new vicious circle, can resolve this contradiction in modern industry [between capitalist relations and the requirements of modern industry] which is constantly reproducing itself.... Certainly to be able to see that the revolutionary elements which will do away with the old division of labor, along with the separtation of town and country, and will revolutionize the whole of production; see that these elements are already contained in embryo in the production conditions of modern large-scale industry and that their development is hindered by the existing capitalist mode of production–to be able to see these things ... it is necessary to have some knowledge of real large-scale industry in its historical growth and in its present actual form, especially in the one country where it has its home and where alone it has attained its classical development (Engels, 1947, pp. 441, 443–44).

All of this suggests that there is something seriously defective about the idea that efficiency must necessarily conflict with unalienated labor. The idea turns out to a corollary of the general theory of technological determinism, a theory which finds little historical support and which rests on the inversion of the relationship between class power and technology. Theoretically, we have little reason to conclude that technological advance and allocative efficiency rule out the effective democratic organization of work, the breakdown of the fragmentation of labor and the corresponding demystification of technology, and working conditions conducive to human well-being and happiness.[11] There is considerable theoretic appeal in Horvat's contention that workers' control and market efficiency are mutually reinforcing, that administrative planning at the macro-level and hierarchical relations within the enterprise "exert a depressing effect on individual performers, stifle initiative, undermine the will to work, cause resistance, in short, lower labor efficiency" (1964, pp. 117–18). Empirically, the balance of evidence points to a positive correlation between efficiency and unalienated labor, although in light of the limited number of studies available on the subject and the rather troublesome methodological problems involved, the question is far from being conclusively resolved. Yet Blumberg's extensive survey of the empirical literature on alienation and participation yields this promising conclusion: "there is hardly a study in the entire literature which fails to demonstrate that satisfaction in work is enhanced or that other generally acknowledged beneficial consequences [including increases in productivity] accrue from a genuine increase in workers' decision-making power" (1968, p. 123; also see Jenkins, 1974, and U.S. Dept. of H.E.W., 1973). Even those

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labor force–to the economic dimension of capitalist society.

[11] Similarly, little theoretical support can be accorded to another platitude in the stock of conservative metaphysics–the idea that there "must" exist a conflict between efficiency and an egalitarian distribution of income. For one of the most recent expositions of this view by an economist and Wall Street consultant, see Arthur Okun's *Equality and Efficiency: The Big Tradeoff.* The title tells it all.

organization theorists who display a penchant for Weberian analysis grant that most studies fail to show that cooperative, participatory, and egalitarian organizational experiments negatively affect efficient performance (cf. Perrow, 1972).[12]

The Market and "Commodity Fetishism." In market socialism, workers control the workplace, utilize the state to uphold the background institutions which justice requires, and adopt market arrangements to secure the efficient allocation of resources. But, says Marx (1906, Vol. 1, p. 84), goods produced for exchange by workers who "do not come into social contact with each other until they exchange their products" are bound to assume a fetishistic quality in the experience of the producers. Commodities take on an independent veneer standing opposed to labor "as an *alien being*, as a *power independent* of the producer" (Marx, 1964b, p. 108).

> The product of labor is labor which has been embodied in an object and turned into a physical thing.... The *alienation* of the worker in his product means not only that his labor becomes an object .... but that it exists independently ... and that it stands opposed to him as an autonomous power (Marx, 1964b, p. 108).

The failure to comprehend the world of commodities as the product of human labor, the inability to pierce the "mystical veil" shrouding the real social relations underlying the forms of commodity production, has as a consequence the domination of human beings ("living labor") by the products of labor ("dead labor"). Social relations themselves appear as relations among commodities or things, alienating people from each other, transforming specifically human relations into narrowly conceived role relations. Marx is clear that "fetishism ... attaches itself to the products of labor as soon as they are produced as commodities, and which is therefore inseparable from the production of commodities" (1906, Vol. 1, p. 83). Commodity production, simple or capitalist, causes alienation.

Now what of the claim that the *market* is a cause of much of this? In market socialism, workers participate in decision making on the basis of equality, move to break down the fragmentation of labor, and operate as cooperatives on the enterprise level. The labor of workers is therefore not "forced," and they are able to derive satisfaction both from the work process and from the products produced for the enjoyment of themselves and others in society. Labor is not alienated from the act of production because the productive relations have been stripped of their capitalist integument. The class obstacles to rewarding work having been removed, workers can proceed to develop freely their mental and physical energies, feel at home at work, and carry on a life of "free, conscious activity." If, at any level of the development of technology, there remain tasks that by their very nature cannot offer satisfaction, their performance can be rotated, or workers who opt to perform them can be compensated with larger incomes or status. At any rate, it is not the market under socialism, but technological inadequacy, which is responsible for the existence of such tasks, and there is no reason to believe that most work under socialism must fall outside the "realm of freedom." There is much to Marcuse's claim that under socialist productive relations and availed of class-liberated technologies,

> the realm of necessity will in fact be changed and we will perhaps be able to regard the qualities of free human existence, which Marx and Engels still had to assign to the realm beyond labor, as developing within the realm of labor itself.... [The rational application of] the material and intellectual forces of transformation are technically at hand although their rational application is prevented by the existing [capitalist] organization of the forces of production (Marcuse, 1970, pp. 72, 64).

Not the market, but class structure and class-determined technological misdevelopment, are responsible for alienated labor.

It's true enough that in market socialism workers produce for exchange and not directly for use, but why *must* production under such an arrangement result in alienated labor, false consciousness, and the estrangement of individuals from one another? Even in a planned economy, regulated by the "whole community," most consumer goods are exchanged for wages, and wages are exchanged for productive contribution, although since net wages are politically determined (as they are in market socialism), labor itself is not a full-fledged commodity. Perhaps the Marxian argument is that people must be alienated unless they fully satisfy their material and psychological needs and that the scarcity of precommunist societies precludes this. But then the problem lies with scarcity, not the market, and it is not at all clear that scarcity implies alienated labor or interpersonal estrangement. If socialist citizens come to some agreement on principles of distributive justice, principles which "provide a way of assigning rights and duties in the basic institutions of society and they define the appropriate distribution of the benefits and burdens of social cooperation" (Rawls, 1971, p. 4), the problem of scarcity could be handled fairly and to mutual advantage, delimiting the incidences of competitiveness, envy, and selfishness. Mandel's apparent belief that scarcity is at the basis of egoism implies his acceptance of the view that most people in societies of scarcity (including socialism) are not capable of a sense of justice (1968, Vol. 2, p. 668). The plausibility of this view is certainly open to question, suggesting as it does that human nature, previous to communist society, is essentially what Hobbes said it is.

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[12] For a good survey of the literature on worker participation, see Greenberg (1975), and Tautsky (1970).

It would seem that if workers succeed in supplanting capitalist control with socialist productive relations, they would at the same time abolish the class basis of "commodity fetishism." With the transformation of work and other spheres of social life (e.g., education, community and political organization), is there any reason to believe that market operation need generate "false consciousness," casting a fog over the social character of production, and subjecting producers to its sway? In market socialism, the working community freely chooses, with a thorough and reasoned comprehension of the automatic workings of market arrangements, to rely on those arrangements for the purpose of allocating economic resources. The idea that production for exchange *must* engender false consciousness is a metaphysical "must," based on a confusion between productive relations and modes of allocation, ruling out a priori the distinct analytical fact and historical possibility of diminished alienation in market socialism.

Concluding Remarks

Selucky (1975) has argued that the rejection of the market is incompatible with the concept of self-managing economic systems: "Any consistently nonmarket economy must be by definition: centralized; run by command plan; controlled by a handful of planners rather than by workers themselves; based on manipulation of producers by the planning board" (1975, p. 58). The idea that the market is necessary to sustain workers' democracy under socialism goes beyond the scope of this article, and it would be inappropriate at this point to entertain an additional and controversial thesis. However, Selucky's treatment of the relation of Marxist theory to the idea of the market is of considerable relevance to a defense of market socialism. He notes that if Marx's account of the market "had been meant seriously, it by no means favors any self-managing economic socialist system," and that "the concept of self-management could not be either accepted or rejected without a substantial revision of the original Marxist theory" (1975, pp. 58, 60). Selucky sees a contradiction in Marx's theory: Marx calls for workers' democracy, control of the political economy by the "direct producers," but Marx iterates the need for central planning which (to Selucky) entails bureaucratic domination. Selucky concludes that "the Marxist concept of the market is far less important for the whole doctrine than is the Marxist concept of revolution" (1975, p. 61). Workers' ownership and control, socialist justice, the historical role of the proletariat are of greater significance in Marx's scheme of things than his critique of the market, and if the former cannot be reconciled with the latter, then the latter must go. "In order to overcome the key contradiction within the Marxist theory, a revision of original Marxist doctrine is unavoidable. Admitting this we suggest that the revision of the Marxist concept of the market is less harmful to the whole doctrine than the revision of the rest" (1975, p. 61). With respect to the claims made in my article, in particular those dealing with socialist justice, I stand in complete agreement with these comments.

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Comedy in Callipolis: Animal Imagery in the *Republic**

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The political society founded by Socrates in the Republic *has been seen by many as Plato's conception of the ideal political community, his Callipolis. However, a study of the language used by Socrates as he builds his perfect city reveals an unusually heavy concentration of animal images. This language seems to undercut the ostensible perfection of Socrates' city and illustrates rather its connections to the comic world of Aristophanes, whose comedy the* Birds *offers the model according to which the* Republic *is built. It is suggested that the city of the* Republic *is comic and ugly, indicating the limitations of politics rather than its potentialities. The* Republic *argues for the need to reorient the concept of justice away from social life and towards the individual. Ultimately, the* Republic *suggests that the notion of social justice is laughable and fit for the comic stage.*

In Book 7 of the *Republic* Socrates gives the city which he has founded with Glaucon and Adeimantus a name. While discussing the education of the philosopher rulers, Socrates says to Glaucon: "It must be established that those in your Callipolis in no way refrain from the study of geometry" (527c).[1] The name, deriving from the Greek *kalos*, suggests that the city is beautiful. The question I would like to raise is whether Socrates' Callipolis is really beautiful, or whether its name may be a deceptive wrapping for what can be considered a political monstrosity which makes its inhabitants ugly and fit for the comic stage. In comedy, the human being portrayed with hyperbolic exaggeration of human weaknesses appears grotesque. Existing on a plane between the gods and animals, human beings frequently become in comedy creatures whose concerns illustrate their ties to the animal world. In tragedy we try to become god-like and fail; in comedy, even as we succeed, we can appear to be only slightly above the animal world. Socrates' city parallels comedy as it transforms the members of its guardian class from individuals with the potential for private virtue into the inhabitants of a barnyard. The entire dialogue which begins and ends with death—that final sign of our inferiority to the gods—is framed by tragedy, as human beings strive to obtain the political self-sufficiency which Plato portrays as impossible. But it is comedy which controls the central part of the dialogue, the growth of Socrates' city in Books 2, 3, 4, and especially 5. The tragic art can make the ugly beautiful; the comic art reveals what is ugly. The externally beautiful polis with the beautiful name appears similar to the wondrous bronze horse inside of which the ancestor of Gyges finds death and the magic ring which will lead him to injustice (359d).

Through a study of the language and metaphor which Socrates uses during his discussion of his supposed utopia, particularly the animal imagery which is used throughout, we find that Socrates' Callipolis imitates the comic art. This ugly city does not reveal how best to organize men and women into political units. It does not clarify the justice of the political system, even the best political system in words, but rather its necessary injustices. Socrates' city is founded on a series of injustices, according to his own definition as it occurs in the *Republic.* He demands injustice to the city's rulers, injustice to its women, and injustice to its neighbors.[2]

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[1] Standard Stephanus pagination will be used for all citations from Platonic dialogues. All translations are my own.

[2] Cf. e.g., 519d, where Glaucon comments that by driving the philosophers down into the cave "we shall be unjust to them and make them live worse lives [*cheiron zein*]." Socrates responds that it is not the concern of the law whether one race "share fare well [*eu praxei*]" (519e). In the last words of the dialogue Socrates enjoins his companions to be just and "fare well [*eu prattein*]" (621d). Women are treated unjustly when Socrates makes them equal to men and thus denied the opportunity to excel in that for which they are most suited by nature. Cf. Benardete (1971, p. 23)

To see Socrates' city as the expression of Plato's political values is to disregard the purpose for which it is founded: to set private justice within the soul in opposition to the justice which the political unit can never achieve. In the process of developing his new definition of justice, Socrates has reoriented the concept from one which may be called political or social, one having to do with an individual's external relationship with others, to a concept which is internal, relating to the soul. Socrates' just individuals do not become just through participation in the polis; rather, they must be made to recognize the inherent injustices in the demands which politics may make of them—to harm the city's enemies (who may in fact be their friends) and to depend on the power of opinion over the truth. The beautiful name becomes a mask which hides the injustices which are a necessary part of politics, even of Socrates' best city.

Comedy in the *Republic*

In the midst of the discussion of the education of the philosopher about which Socrates cares most, he catches himself: "I forgot that we were playing [*epaizomen*]" (536c; cf. 545e). The playfulness of the dialogue is frequently expressed by laughter. Cephalus is the first to laugh as he leaves the group assembled at his house (331d). The laughter which he bequeaths to the group along with the argument pervades the dialogue, despite Adeimantus' early plea that Socrates demonstrate that one hearing justice praised should not laugh [*gelân*] (366c). Throughout, Socrates as he tries to show why justice must be praised is himself deserving of laughter [*geloios*] (392d; 398c; 432d; 445a; 499c; 504d; 506d; 536b). Glaucon, his companion in this quest, similarly appears laughable, particularly when he tries to relate the value of various sciences to the mundane problems of war and politics (526d–527a; 527d; 528d–529c; 529e–530a).

In the *Philebus*, Socrates enumerates the causes of the laughable [*to geloion*]; he finds them in three variations of our failure to follow the Delphic maxim "know thyself." We become the source of laughter when we do not recognize our limitations with respect to wealth, beauty, and virtue (48c–e). By building his supposedly beautiful city Socrates is guilty of the second offense against Apollo, becoming a source of laughter for presenting as beautiful what is clearly ugly. It is only in Book 10 that the unjust man and not Socrates becomes laughable (613d; 620c). Once the best city is left far behind, laughter no longer plagues the arrogant Socrates or the philosopher who is forced back (as Socrates is at the beginning of the dialogue) into the cave of the political world (517a,d). Only at the end of the dialogue is Adeimantus' request fulfilled that justice not be made laughable. Previously, the attempt to praise justice by uniting politics and philosophy and by making the philosopher Socrates engage in the political activity of founding a city only rendered the discussion of justice more laughable.

In Socrates' city, laughter, if not totally eliminated, is circumscribed. Homer is censored for portraying the gods as susceptible to "unquenchable laughter" (389a). The training of a good warrior accomplishes control over the warrior's emotions—and this includes control over laughter. The warriors and guardians by becoming divine must not change form. "It is necessary that they not be lovers of laughter; generally, whenever someone laughs violently, such a one seeks a violent change" (388e). All are prohibited even from imitating one who laughs. Yet, there is much laughter as Socrates founds his city, a fact too seldom recognized in our awe before the venerable philosopher.[3] Socrates himself introduces comic elements in his very language (Jowett and Campbell, 1894,

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and Saxonhouse (1976, pp. 206–11). Neighbors whose land must be acquired to support the non-farming population are injured and their land taken away (373d).

[3] Jowett and Campbell (1894) comment in the note to 563a: "The most extravagant and comical ideas often occur in the works of Plato. But the manner of saying them does away with the feeling of bad taste." The sensitivity which Jowett and Campbell frequently show in their notes to the Greek text unfortunately does not influence their general analysis, nor Jowett's translation. Rosen (1964, p. 460, cf. p. 464) in contrast does recognize the humor in his analysis of the *Republic*, "making the same suggestion about the *Republic* that Spinoza and Rousseau made about Machiavelli's *Prince:* that it is a kind of satire whose exaggerations are meant to teach the opposite of what they explicitly say." Though I do not agree with several of Rosen's conclusions, I do value the attempt to look at the satire and the humorous goals of the *Republic.* See also, Strauss (1964, pp. 51–52, 61 and passim). On comedy in Plato in general, see Klein (1964, pp. 4–7) and Greene (1920). Greene (1920, p. 101, cf. p. 97) concentrates primarily on the comedy of language, the unusual or unexpected metaphor, not on the comedy of action, and sees the "injecting of detail in a serio-comic vein" as the "method of filling the *lacunae* that are bound to exist between actual and ideal conditions."

pp. 116, 160), and in Book 5, despite all of Socrates' admonitions, the inhabitants of his best city, though they themselves do not laugh, enact their own comedy and cause others to laugh. The comedy in this book is expressed by the explicit laughter which surrounds Socrates' proposals for the social structure of his society and, as we shall see below, by the relationship between these reforms and the humorous reforms found in the plays of Aristophanes.

Book 5, until after the introduction of the philosopher king, is filled with laughter, or the mock fear of it. Socrates begins his own discourse on the topics of sexual equality, communism, and philosopher rulers with admonitions as to his own doubts (450c); the proposals set forth are to be taken as most tentative. He does not fear, he says, any laughter [*ti gelôta*], for that would be childish (451a). Glaucon responds by laughing (451b). Socrates is laughable because his proposals are opposed to convention, as he himself explains (452a). Nevertheless, he next proceeds to suggest what will be most laughable of all: naked women practicing alongside naked men in the palaestras (452a–b). Glaucon, swearing by Zeus, agrees that it would be laughable in the present state of affairs and Socrates repeats that they must not fear the jokes which clever men will make about such a sight.[4] Socrates begs those who find it laughable not to treat as laughable that which is opposed to what is customary; rather, he argues, the laughable must be defined by the criterion of good and bad (452c–e). Socrates, however, has yet to prove that the naked female engaging in gymnastics is good. The process of this proof is dubious, and it is based on a prior understanding of the good, the bad, and the laughable, since all arguments in opposition to his theory are discarded as laughable (454c; 455c–d; 456d; 457a–b).

The biggest joke of all in Book 5 is the proposal for the philosopher ruler. Socrates realizes that this proposal is likely to drown him in a wave of laughter (473c), and in Glaucon's violent reaction to this proposal (473e–474a) "we are reminded of the manner in which the upholders of paradoxical or revolutionary ideas are threatened with popular hostility in Aristophanes' [comedies].... The real solemnity of the revelation is instantly broken by the ludicrous outburst which follows ... relieving the discourse by ludicrous imagery" (Jowett and Campbell, 1894, p. 254). After Socrates makes his outlandish suggestions in Book 5 the laughter fades away, only to surface again at the beginning of Book 7 in the allegory of the cave. There, the philosopher returning from the light of the sun to the shadows of the cave appears laughable because he does not understand the conventions of the cave (517d). The philosopher is laughable because he is outside society and its conventions.

The Platonic dialogues are humorous; they mock both the characters within the dialogues and the readers who are drawn into the dialogue. We cannot isolate the famous Socratic irony from Plato's own comic art. Plato's humor, though, is not an arbitrary literary flourish. Plato is a literary artist as well as a philosopher, but his literary skill serves his philosophy, and the playful games he invents serve the propaedeutic purposes of the dialogue (Klein, 1964, pp. 4–5).[5] Recently, considerable attention has been given to the dramatic quality of Plato's dialogues and to the integration of the action of the dialogues and the philosophic content (e.g., Bloom, 1968; Klein, 1964; Strauss, 1964).[6] Similarly, we must study how comic elements frequently expressed through language and metaphor appear at certain points in the dialogue and illuminate the philosophic content. However, before we can understand the role which this humor plays in the dialogue, it might be helpful to refer to earlier Greek literature to which the *Republic* may be a reaction, particularly the work of Aristophanes.

Plato and Aristophanes

Plato's desire to use the dialogue as an educational device is hindered by the Greeks' devotion to the poets of the past and present, the poets who provide values and belief systems for the Greeks. When Cephalus, Polemarchus, Glaucon and Adeimantus talk about justice, they talk about the justice which the poets have described for them (331a; 331d; 362a–b; 363b–c; 364c–e; 365b). Though Socrates attacks most harshly the poetry of Homer, it is clear that the epic poet does not have exclusive control over the education of the young. Those who write for stage productions, those who do not use the intermediary narratives of "he said" and the like, are poets and educators as well. These poets must also be overthrown by the prose of Plato's dialogues if his new education is to succeed. The poets who write without the narrative parts can be writers of tragedy or of comedies. Socrates clarifies this in his correction of Adeimantus, who at first recognizes only tragedy (394b–c; 394d). There were three major tragedians in the Periclean Age and they all receive hostile mention in the *Republic.* Sophocles appears as one who was overly erotic in his youth (329b–c). Aeschylus blasphemes the gods (380a; 383a–b). Euripides encourages tyranny (568a–b). While the three tragedians are discredited by name, among the writers of comedy there is only one who has such preeminence. This is Aristophanes. Yet in the *Republic* he remains unnamed, though like the tragedians and epic poets he remains one of Plato's most serious rivals in the education of the youth. Plato remains silent about Aristophanes because of the use which he is to make of him, overcoming him by appearing to ignore him (Bloom, 1968, pp. 380–82).

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[4] Jowett and Campbell (1894, p. 225) comment: "Jests about the gymnastics of the Spartan women such as Plato describes are found in the *Lysistrata* of Aristophanes (80–83)."

[5] For the importance of play as a tool for education, cf. 424d–e and 537a. In the *Laws*, the discussion is often described as a sort of game: 685a; 712b; 769a; and games are used as educational devices: 797a–798e; 819a–d.

[6] Cf. Klein (1964, p. 4, n. 10) for a limited bibliography from the early nineteenth century through the middle of the twentieth century, as well as his introductory remarks (1964, pp. 3–31) for a full exposition of the value and necessity of this approach.

The comedy by Aristophanes which is important for our consideration of the *Republic* is the *Birds.* Here, two Athenians leave Athens to find a commodious and pleasant place in which to live, one which is free from the tribulations of Athens with its plethora of sycophants and trials. What they seek, it turns out, is the natural city, one which accords with natural desires and needs, where one may act without the inhibitions imposed by conventional society. These men find their natural city, devoted to the pursuit of pleasure, among the birds. There they find no money, no private property, no servants, and complete sexual freedom for both heterosexual and homosexual activities. Aristophanes suggests that the bird society is the natural society. Behind this comedy is the contemporary intellectual conflict between the demands of nature and the restrictions which conventional society imposes on human activity.[7] Socrates tries in the *Republic* to create the natural city in which natural justice will be found. As the city is founded in Book 2, nature [*physis*] is the criterion (369a–b; 370c). The city of nature is Socrates' true city (372e). For Aristophanes, as for the Sophists of his time, the natural city is one based on hedonistic pleasure; in the *Republic* Socrates adapts the sophistic ideas by trying to find justice, the true source of pleasure, in nature.

As Aristophanes' comedy progresses, one of the Athenian travelers organizes the birds into a polis. He changes the natural life of the birds into a city based on convention, having a name and artificially organized into leaders, workers, and messengers. In the same way Socrates turns his natural polis into a conventional society as he organizes his own flock of animals into a highly structured city with its own three classes. The Athenian of Aristophanes' comedy continues to build defenses and ultimately deposes the Olympian gods; Socrates does the same in the *Republic,* establishing the military class of warriors and the guardian rulers and in his own way deposing the gods of Olympus through his reforms of poetry and the replacement of the old gods with his "ideas." In Aristophanes the laughter comes from the fantasy of the episode, the absurdity of human beings founding a city among birds, and from the appealing freedom from standard conventions which such a society might offer. In the *Republic* the laughter comes as Socrates, rejecting that which is habitual, tries to found a city with animal inhabitants; but it comes as well in the reaction to the fantasy of the just city, an ideal which has for Plato some of the same absurdity and yet initial attraction which the city of the birds has for the Athenians.

The relationship between the *Republic* and Aristophanes' other utopian comedy, the *Ecclesiazusae,* has been the subject of discussion for well over two centuries.[8] Similarities in the communistic programs, both economic and social, and the introduction of women into the ruling classes suggest a close link between the two, but the significance of the link has seldom been considered with attention to the attendant comic interrelationships. If it is funny in Aristophanes, why isn't it funny in Plato? If it is a comedy when Aristophanes inhabits his utopia with birds, why isn't it funny when Socrates inhabits his with dogs? If equality between the sexes is funny in Aristophanes, why isn't it funny in Plato? Although Plato does not intend to be the comic artist that Aristophanes is, neither can he be read without an awareness of Aristophanes' literary career. There are too many references throughout the Platonic corpus to Aristophanes to pretend that Plato would have been oblivious to Aristophanic themes and the uses which the comic poet makes of them. Though the *Republic* does not provoke the open laughter that Aristophanes' comedies do, the comic themes cannot be ignored.[9]

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[7] Cf. esp. Fragment 44, "On Truth" by Antiphon the Sophist in Freeman (1971, pp. 147–49).

[8] Adam (1902, Vol. 1, pp. 345–56) gives a detailed history of the arguments; also Ussher (1973, pp. xvi–xx); and most recently Bloom (1977, pp. 324–28).

Animal Imagery in the *Republic*

With this literary background, one can begin to look at the language of the *Republic* from a new perspective. As one goes through the *Republic* noting the animal images, one is struck by the preponderance of such images; and frequently it is this imagery which highlights the humorous aspects of the dialogue as Plato rewrites Aristophanes' *Birds*. The animal imagery appears most frequently in Book 5, the most comic book, where Socrates elaborates upon the social and political structure of his best city, but it begins already in Book 1 with the dramatic treatment of Thrasymachus.[10] In the actual discussion with Socrates Thrasymachus introduces a form of animal imagery which can hardly be considered unique in Greek political thought: that of the political leader as shepherd of his flock. Thrasymachus finds fault with Socrates' putative nurse for failing to point out the difference between the sheep and the shepherd. "Do you think that shepherds and cowtenders look to the good of the sheep and cows, and fatten them and care for them, looking for anything else than the good of their masters themselves?" (343b) Socrates, of course, twists the intent of Thrasymachus' statement and shows that the shepherd is indeed concerned with fattening his sheep for the sheep's sake, and that likewise the true political leader must take the best possible care of his subjects. By introducing the shepherd model, Thrasymachus is going back at least as far as the Homeric formula where the expression *poimnê laôn,* the flock of the people, entailed within it the concept of the ruler as shepherd.[11]

The shepherd imagery which appears elsewhere to describe the political relation of ruler to subject, however, is not the central image of the *Republic.* On two occasions the city is described as the flock protected by the shepherd and his dogs (416a and 440d). In the latter instance it is Glaucon who establishes the analogy, to which Socrates replies: "You understand well what I wish to say." But it is not within the benevolent shepherd model that the best city of the *Republic* is discussed. If it were, we might have no cause to see anything unusual in the animal imagery. Shepherd imagery in the *Republic* appears in terms alien to the traditional picture of stability and protection. Concern in each case is expressed about the shepherds' ability to restrain their dogs from harming the sheep which they are tending. Such problems are not inherent in the traditional model. The variation on this theme must call attention to the peculiarities of Socrates' use of these animal images.

In the sections of the dailogue which do not deal with the ideal city, the political world is described in animal terms as well, with decisively derogatory connotations. The philosopher in the city is compared to a man who has fallen among "wild beasts" and who is unable to withstand their savagery (496d). In another parable, the *demos,* the core population of the democratic state, is compared to a wild beast which must be petted and accommodated so as to make it most pliable (493a–c). The images of a savage political community, emphasizing the bestiality of the ignorant men who comprise and rule this community, underscore Socrates' disdain for conventional politics. But when he turns his own city into a community of animals we find that the difference between this polis and the one which is soon to kill him is not the quality of the human being, the

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[9] Another possible earlier influence on Plato may have been those pre-Socratic philosophers called by Havelock (1957, Ch. 5) the Greek anthropologists. Such authors as Anaximines, Anaximander, Anaxagoras, Xenophanes, and Archelaus treated man as simply another animal whose origins paralleled those of other animals. Frequently, it was speculated that men were even born from other animals. This man studied by the pre-Socratics had no distinctive *nous* or *psyche* which distinguished him from other living beings. Cf. Fragments 136, 137, 139, 140, 172, 184, 185, 534, and 542 in Kirk and Raven (1957). However, our knowledge of pre-Socratic thought is so limited by the fragmentary condition of their writings, that it can only be tentatively suggested that Plato may be reacting to their equation of man with animal as well as to Aristophanes in the *Republic.*

[10] Cf. 336b, d; 341c; and 358b for the portrayal of Thrasymachus as an animal. The taming of Thrasymachus is suggested not by reference to the Sophist himself, but to the animals with which he is associated. Cf. 411d. In Books 8 and 9 the animals of Thrasymachus reappear, recalling the earlier role of Thrasymachus as the potential (though far from complete) tyrant whose soul is now laid bare to reveal the internal condition of what was previously seen only as a savage and bestial exterior. Cf. 566a; 588c; and 590b. Though the animals of Thrasymachus are not directly related to the theme of comedy in the *Republic,* they do suggest the value of focusing on such images as they recur throughout the dialogue.

[11] Louis (1945, p. 162) counts 41 instances of this phrase in the *Iliad* and 10 in the *Odyssey.* He also cites Hesiod's *Theogony* 1000 and Euripides' *Suppliants* 191.

anthropos, who inhabits it, but merely in the distinction between the tame and the wild beast.

The founding of the just city begins in Book 2 and almost immediately the barnyard imagery intrudes. The first city, the true city, is one founded on needs, egoism, specialization, and craft. It is a city of human beings performing that task for which they are most suited by nature. It is a city in which pleasures are defined by the straw mat to sleep on, toasted acorns, and a life spent in peace. It is, as Glaucon phrases it, "a city of swine" (372d) whose inhabitants Socrates "fattens." The word used for fattening [*chortazein*] is one properly applied to cattle who are being fattened in a stall, not to human beings who may be fed, but usually are not fattened as though for slaughter (Jowett and Campbell, 1894, p. 87; Liddell and Scott, 1968: ad loc.).[12]

The fevered city of luxuries, which emerges as the result of Glaucon's outburst, includes meat as well as fodder. It is here that we find animal behavior providing a model for human behavior. The second city requires more land than the first and needs protection from foreigners who desire its luxuries. Thus, the military class, the warriors, are introduced and in order to understand their prospective natures and functions, we turn to animals, particularly dogs and horses. The imagery begins as Socrates says to Glaucon: "Do you think that there is any difference in nature between a well-bred [*gennaion*][13] puppy and a well-born youth with regard to guarding?" (375a)[14] Glaucon, not always so ready to follow Socrates' lead this early in the dialogue, asks what Socrates means to say. Socrates explains: "Well, surely it is necessary for both of them to have a sharp sense [*aisthêsin*] and be nimble at pursuing what they perceive, and furthermore be strong, if it is necessary to do battle with what they have caught" (375a). *Aisthêsis* has a special use in hunting where it indicates particularly the dog's ability to pick up the scent of the pursued animal (Jowett and Campbell, 1894, p. 93). Glaucon accepts this analogy and therewith the analogy between puppies and youths. From now on Socrates can freely use these analogies with Glaucon to carry forth the discussion.

The next question relies on the undisputed assumptions of the analogy. Socrates is anxious that his warriors be brave. "Is the one lacking spirit whether a horse or a dog or any other animal likely to be brave [*andreios*]?" (375a) *Andreios*, the Greek word for masculine courage, entails within it the traditional, aristocratic conception of virtue. It was the military heroes of the past, the brave men who stood on either side of the Trojan walls in Homer's epics, who had offered the Greeks their models of virtue until this time. Now these men are equated to noble puppies, dogs, or horses—hardly images which call to mind the greatness of an Achilles or a Hector. When the heroes of the *Iliad* were compared to animals it was to lions or boars, animals which symbolized power and violence.[15]

The analogy with puppies continues to serve as a source of supposed enlightenment. "How," asks Socrates, "will they not be savage [*agrioi*][16] with one another and with the other citizens, being of such a nature [high-spirited]?" (375b) How can one have gentleness and spirit in the same individual? Socrates describes himself as being at a loss [*aporêsas*], a word repeated twice in this short passage to emphasize the difficulty of the situation. Thus, the resolution takes on greater significance. They were "justly" at a loss, for "we deprived ourselves of the simile [*eikonos*] which we proposed" (375d). The *eikon* is the noble puppy.

> One would see in other animals too, but not least in the one to which we likened the guardian. For you know concerning well-bred dogs that this is their character by nature, to be as gentle as possible to those who are familiar and known, but the opposite toward those who are unknown (375d–e).

Canine behavior becomes the model, not only the simile, for human behavior. Dogs show that it is not against nature [*para physin*] to search for a unity of gentleness and fierceness in one individual. Thus, we get guardians who will be fierce with their enemies and gentle with their friends.

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[12] Cf. 586a for the only other use of this term in Plato. Here it refers explicitly to animals, as Socrates describes those who live in a world of false pleasure "in the manner of cattle, always looking down with their heads towards the earth and the table, they eat, are fattened [*chortazomenoi*] and mate."

[13] *Gennaios* connotes nobility of breeding and is usually applied to humans, but Liddell and Scott (1961: ad loc.) citing Plato say that it can also be used for animals.

[14] Adam (1902, Vol. 1, p. 106) suggests that a play on *skulax* (puppy) and *phulax* (guard) is intended.

[15] E.g., *Iliad*, Bk. 2, l. 23; Bk. 4, l. 471, Bk. 5, 11. 136, 161, 299, 782; Bk. 7, 11. 256–57; Bk. 8, l. 337 (here Hector is like a hound, Achilles like a wild boar or lion); Bk. 10, 11. 295, 485; Bk. 11, 11. 113, 129, 172–75, 383; Bk. 12, 11. 42, 146, 293. 299; Bk. 13, l. 198. Cf. Redfield (1975, pp. 189–203).

[16] The word *agrios* appears frequently in the *Republic*, far more so than in any other dialogue. Though it does not refer exclusively to animals, the term primarily connotes living in the fields or open spaces, hence wild and savage (Liddell and Scott, 1961: ad loc.).

Socrates goes so far in this analogy as to make dogs not only human, but even philosophic. In Book 1 Polemarchus had tried to defend Simonides' definition of justice as helping friends and harming enemies, but had retreated before Socrates' suggestion that one does not always know who is friend and who is foe. Simonides' definition floundered on an issue of knowledge. Lest this problem plague the founding of the best city, Socrates makes his guardians into philosophers who know their friends and enemies, defined purely by the criterion of familiarity. To do this, he turns again to animal behavior:

> This also you will see in dogs, and it is a cause to wonder at the beast.... When he sees one who is unknown, he is harsh although he has experienced no harm. But the one who is known he welcomes, even if he has never experienced any benefit from him. Or did you not yet wonder at this? (376a)

Glaucon remarks that he had in fact not yet wondered, but acknowledges it now and goes so far as to accept Socrates' notion that we can call dogs on this account philosophic.[17] Later in the dialogue philosophy will come to have a very special meaning for Socrates which can only be understood in terms of human endeavor and potential and in terms of the rational faculty. Here, though, in the political development of the best city, the human watchdogs are admitted to the ranks of philosophers since their canine counterparts can differentiate between the good and the bad on the grounds of the known and unknown.[18] "Shall we, being bold," asks Socrates, "assume also in a human being that it is necessary if he intends to be gentle towards his own and those who are known that he be by nature a philosopher and a lover of learning?" (376b–c) The boldness came earlier when dogs were ascribed the characteristics of those whom Socrates regards as engaged in the highest human endeavor. The political community demands Simonides' definition of justice. Friend is defined by fellow citizen, whether that person is truly a friend or not. Philosophy as the endeavor for wisdom is the pursuit of the unknown. The city which relies on traditional customs cannot accept the unknown. By bringing philosophy into the realm of politics, it is prostituted and made the possession of mere brutes. Simonides' definition is apt for the animals who inhabit the political world of the city.

As the discussion of the guardian class continues through Book 3, where Socrates turns to the education of the warriors and the purging of the poets, the animal images and the dog analogies are maintained, though not with the frequency of Book 2 or Book 5. The educational process is meant to turn the guardians into perfect watchdogs, to make them submit to the public sphere and forget their private needs. They are to be made void of personal desires and emotions, void of individuality. Their relationship to the political community is their only defining characteristic, just as the watchdog is defined by its relationship to the sheep which it protects. The training of the guardians is a taming process, one which while making them strong psychically and physically will make them obedient to their rulers.

The guardians must be made gentle, *hêmeros*, a word which may be applied equally to human beings and to animals, but which has its etymological roots in animal behavior.[19] In Book 2 the founders of the just city focused on courage for which dogs served as the model. Untamed courage, though, such as Thrasymachus displayed, can become savage. In Book 3, thus, attention is turned to moderation, the middle ground between being too savage and being too soft (410d–e). The education of the warriors must moderate both extremes and create a tame guardian, one who is not weakened by philosophy, nor made savage by gymnastics. The personality is no longer split in its orientations towards insiders and outsiders; education integrates the warrior into a tame animal. The major concern, though, is not the weakness brought on by too much philosophy, but the savagery which must be moderated. "It would be most awful and shameful for a shepherd to so raise the dogs as guardians of the flock that by wantonness or hunger or any other evil disposition, the dogs try to harm the sheep and become similar to wolves rather than dogs" (416a). The education of the warriors is to ensure that they do not turn into wolves instead of dogs, savage tyrants [*despotais agriois*] instead of allies (416b). It is education which tames these potential tyrants, just as Socrates tames Thrasymachus. It is education which the wolves of Book 9 lack as they taste of human blood, and it is this education which enables the warriors to be brave. The uneducated does not possess courage; his passion is like that of a wild beast. The trained puppy can be *andreios*; the wild boar cannot. The Homeric heroes displayed the courage of wild beasts. Socrates' new breed of men possesses the controlled vigor of the domesticated horse and dog.

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[17] Sinclair (1948, pp. 61–62) argues that "we must not take Plato's little jokes seriously." He sees in Plato's discussion of the philosophic dog, a parody of the "method of argument used by the 'nature' school of sophists, who advised that men should follow *phusis* not *nomos*. The notion that observation of nature, especially of the animal world will show what is the way for men to behave was taken seriously." Adam (1902, Vol. 1, p. 108) suggests that perhaps this is an allusion to the Cynics, "who were called Cynics because they welcomed and were friendly to those who followed their pursuits, but hostile to those who were opposed."

[18] This definition will come back to haunt Socrates when he tries to put women into the guardian class and asks his interlocutors not to laugh at the unknown (452b–c). By accepting the unknown are Glaucon et al. showing themselves to be bad guardians?

[19] Liddell and Scott (1961:ad loc.) define *hemeros* as "tame, tamed, reclaimed, of animals, opp. to wild, savage." The metaphorical use applies to men as gentle and kind.

In order to turn the potential wolf into a tamed dog, every aspect of its education must be controlled, from diet to sexual relations. The careful supervision exercised over the poets extends also to the craftsmen.

> The one not able [to avoid bad qualities in his workmanship] must not be allowed to practice in our city, so that our guardians may not be raised amidst bad images, as if on bad grass [*botanê*, fodder, pasture], plucking and grazing on much each day, little by little, from many places, drawing together one big evil in their soul (401b–c).

The warriors here are like human cattle. They match the surroundings in which they are raised and feed on education in the same indiscriminate way that cattle feed on the grass in their pasture. Socrates suggests that good natures nurtured on good grass or on good education "become still better than those before, in other things and in the breeding process, just as among other animals" (424a–b). The quality of the race depends on the quality of their fodder.

Within the educational scheme proper, the guardians are trained like animals and encouraged to become animals. It is necessary for the warriors to become "as wakeful as dogs and to see and hear as sharply as possible" (404a). The potential guardians are watched so that only those most suited to the arduous tasks of guardianship remain among the warrior class. Part of the selection process includes a test of one's own powers against the powers of magic; "just as they see if colts are fearful by leading them to noises and confusions, so too while they are young they must be brought against whatever is terrible" (413d). Once the warriors have been properly trained and selected, the rulers will lead them forth and they will look for the fairest spot in the city to set up camp. "From there they would restrain those from without, if any enemy such as a wolf should come down upon the flock" (415d–e). As well-trained sheep dogs, they direct the sheep and defend them against predators. This image is carried on vividly at the beginning of the next book. Socrates' city will be safe from attack; neighboring cities would not choose war with the lean dogs of Socrates' city (422d). The lean dogs or warriors after setting up their camp, sacrifice and settle down to sleep in bedding [*eunê*] (415e), a word which may refer to the place where an army settles as well as to the lair of a lion or the nest of a bird.

Though animal imagery of this sort persists throughout, it appears most frequently in Book 5. Book 5 also contains the most frequent laughter.[20] On almost every page Socrates' suggestions are seen as laughable. Whereas previously the animal imagery may have been merely curious or mildly disturbing, in Book 5 we find that it is meant to be funny. The fifth book itself is offered as something of a detour. Socrates has discovered the just soul and is about to prove that this soul is happy while the unjust soul is not. He plans to do this through a discussion of the degenerate cities when the dialogue suddenly begins over again[21] and Socrates is forced to discourse on the social structure of his ideal city. He does so under compulsion, with many doubts[22] and with the assurance that he will be treated as guiltless, like the man who commits an involuntary murder. It is with these precautions that Socrates confronts the three waves of Book 5: (1) the equality of the sexes; (2) communism and the community of wives; and (3) the possibility of such a city ever existing, which turns into the issue of the philosopher king. For the first two waves, animals solve the paradoxes and serve as models for human behavior in the political world.

At the beginning of Book 5, Socrates realizes that thus far he has only dealt with the male act, with the possession and use of children and women, and that he has set up only men as "guardians of the herd" (451c). But if we look at the "female of the guardian dogs" we discover that they "guard and hunt together [with the males] and do other things in common" (451d). They do not stay indoors "as incapable on account of the birth and rearing of puppies, while the males work and have every concern about the flock" (451d).[23] If there is sexual equality among dogs, why not institute it among the members of the city who thus far have been treated primarily as dogs? Women become guardians because "remaining in the city or going out to war they [women and men] must guard and hunt together just as dogs [*hōsper kunas*]" (466c–d). In fact, Socrates argues that "there is no pursuit relating to the governing of a city which belongs to a woman because she's a woman, nor to a man because he's a man, but the natures are scattered among both animals [*en amphoin toin zôoin*]" (455d). Equality of the sexes is asserted even though the female gives birth while the male "mounts" or "covers" (454e). The term used for "mount" or "cover" by Socrates is again one that applies in Greek only to animals.[24] Though Socrates in these pages is talking about the sexual equality of the inhabitants of his ideal city, it is sometimes hard to distinguish them from animals.

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[20] There are at least 20 uses of some form of *geloios* within the first 35 Stephanus pages of the fifth book.

[21] Cp. 449b with 327b.

[22] Socrates emphasizes his own hesitation by three references to his doubts in 450c.

Once the equality of the sexes is proved with analogies to the animal world, it is necessary for Socrates to provide the same education for both sexes, since "to use any animal for the same things . . . you must give them the same nourishment and education" (451e). This equality of education means practicing gymnastics together in the palaestra, ignoring sexual differences, just as animals do except during the breeding season. The equation of men to animals and women to men leads immediately to laughter and a series of jibes.[25] Though Socrates and his companions agree that the fault belongs to the laugher, nevertheless comedy and the absurd enter the discussion–and the mind of the reader of the dialogue. In arguing for the equality of the sexes Socrates is presenting a notion so alien to Greek thought[26] that it is fit only for the comic stage. Socrates in part captures the humor of the notion through his animal images, by making men look to the animal kingdom for the model of sexual equality.

This sexual equality, however, creates difficulties for Socrates' city. The Greek family, based in large part on the invisibility of the female outside the home and on the female as the means of transferring property, no longer offers a model for patterns of procreation and education. The traditional pattern must be replaced with a radically new one, and once again the model comes from animals. Although procreation had not previously been considered a part of the political dialogue and the female's role as the bearer of children had been ignored, now that the traditional family had been destroyed, political control over procreation fills up the middle section of Book 5. "Tell me this, O Glaucon, for I see in your house both dogs for hunting and a large number of well-bred birds. Have you ever noticed anything with regard to their marriages and child making?" (459a) Glaucon is uncertain, as well he might be, as to what Socrates is suggesting. Socrates explains that he is referring to the careful breeding of these animals and how one does not breed birds, dogs, horses or "the other animals, except when at their prime." "Aha, dear friend," Socrates exclaims, "how much is it necessary that we have rulers of the highest quality if it is the same about the race of human beings" (459b). The rulers then are to breed their guardian class as if they were dogs or horses, raising only the offspring of the best "if the flock is to be the most excellent" (459e).[27]

The rulers' manipulation of the breeding process, however, must be kept secret so that the "herd of the guardians may be as free of conflict as possible" (459e).[28] The secrecy is to be accomplished by the drawing of lots which match the selected guardians. Once the partners have been matched, they do not retire to a private bedchamber. Rather, they are shut up together as if the rulers were breeding cattle or dogs. The word *sunerxis* used twice in this short passage (460a and 461b) to indicate the enclosing of the mating couple is one "properly used of penning animals" (Jowett and Campbell, 1894, p. 230).[29] The mates in this union, as Socrates learns from his observation of Glaucon's pets, are to be in their prime. For a woman this means from her twentieth year until her fortieth year; men shall engender children from the time when they are beyond the "fast prime of running" until the fiftieth year (460e). Adam (1902, p. 310) suggests that the phrase "the swiftest prime of running" is a poetic formulation which was probably not applied to a man, but to a race horse. He argues that the "comparison gains in realism and point, if it was the custom of antiquity, as it is now, to bring a first-rate racer to the stud . . . when he ceased to run." (The animal imagery here may have been carried beyond simply language to poetic echoes which are difficult for a modern reader to recognize.) As soon as the offspring from these unions are born, they are taken to a pen [*sêkos*] to which the mothers are led when they are full of milk (460c). Jowett and Campbell (1894, p. 231) refer the reader to the use of *sêkos* in *Odyssey* 9 (219 and 227) to describe the place where lambs and kids await their mothers to be fed. Thus, it occurs as well in Socrates' own barnyard. While the adults breed as if they were animals, the offspring are treated as if they were lambs or kids.

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[23] Jowett and Campbell (1894, p. 218) comment that the words *hê anthropinê hê thēleia* of 452e "keep up the analogy between man and the other animals which runs through the passage."

[24] Liddell and Scott (1961, ad loc.) comment: "It seems to have been the generic word for all animals. . . ; but was not properly used of mankind, though in Pl. R. 586a it is used of men like beasts." Cp. 586a and above n. 12, where the word for "mate" is also *ocheuô*.

[25] 452a (twice); 452b; 452c; 452d [*kômôidein*]; 456c (four times); 457a; and 457b (twice).

[26] The question of the role of women in Greek society is a much-debated issue, but see esp. Arthur (1973) and Pomeroy (1975).

[27] Jowett (1892, Vol. 3, pp. clxxxi–clxxxii) comments: "There is no sentiment or imagination in the connections which men and women are supposed by him to form; human beings return to the level of animals, neither exalting to heaven, nor yet abusing the natural instincts. . . . The analogy of animals tends to show that mankind can within certain limits receive a change of nature. And as in animals we should commonly choose the best for breeding, and destroy the others, so that there must be a selection made of the human beings whose lives are worthy to be preserved."

When Socrates turns from procreation to the military training of the members of his city, the animal images continue. The young are brought close to battle because "every animal [*pan zôon*] fights eminently when those are near whom it has borne" (467a–b). That neither the women nor the men know which children they have borne or sired is ignored. Later we learn that the children "if it is safe anywhere, must be led near [to the battle] and must taste blood, just like puppies" (537a). The young who may watch the battles are also compared to birds; it is necessary that they be "winged straightaway as children," so that if need be "they may flee by flying" (467d). The wings, as it turns out, are to be the horses whom the children must learn to ride, but Socrates chooses to elaborate on this theme with a metaphor–one which underscores his own continuing interest in the animal images. The treatment of military procedures in the fifth book ends when Socrates and Glaucon agree that their city will not pay a ransom for any guardian who is captured alive by the enemy. Rather, they will offer the "catch" as a gift to their captors to deal with as they wish (468a). The word for "catch" is *agra*, one used to describe the animal which has been taken in the hunt.

As Socrates turns to the third wave of Book 5, the question of feasibility, he asks whether "it is possible among human beings [*en anthrôpois*], as among other animals [*en allois zôois*] that there be this community, and in what way it is possible" (466d). The phrasing of the question suggests that animals are able to achieve this social structure and that the human being is asked to model his society after that which the animal kingdom has naturally followed. The conclusion will be, as is well known, that this parallel is realizable if, and only if, philosophers become kings or kings become philosophers; i.e., with philosopher kings we can create the organized polity of animals. The section dealing with the question of feasibility and subsequently the philosopher is largely free from the animal imagery so prevalent in the earlier sections of the book. However, even the philosopher kings on at least two occasions do not escape description in animal terms. "When strength leaves them and they are far from political and military affairs, let them be released to go out to pasture [to graze, *nemesthai*] and do nothing else that is not a pastime" (498b–c). The veteran philosopher kings like old cattle are sent out to the fields when they are no longer of service to the polity. Even these venerable old men and women who have pursued the philosophic education and are headed up out of the cave to the world of being become through their involvement in Socrates' utopia comparable to old cows or horses. The obligation which these rulers have towards the city is later discussed in the language of bees. Glaucon shows concern about forcing the philosopher down from his contemplation of the good into the shadowy world of politics. Socrates responds that the philosophers must be told: "We have begotten you for yourselves and for the rest of the city as if you were the leaders and rulers in a hive" (520b).[30]

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[28] Jowett and Campbell (1894, p. 229) and Adam (1902, Vol. 1, p. 298) comment that the use of *agelê* here is intended "to recall the analogy of the lower animals."

[29] The only other appearance of this term in Plato is in *Timaeus* 18d, a clear reference back to this passage in the *Republic*.

The Body and Politics

After citing all of the examples of animal imagery in Socrates' ideal city, we must ask why he makes his citizens equal to animals. Why has he chosen to undercut what has appeared to so many as a beautiful ideal by imposing an Aristophanic theme? A few of the many commentators on Plato and on the *Republic* have tried to come to terns with his use of such language. Rankin (1964), Adam (1902), and Sinclair (1948) all recognize Socrates' concern with nature [*physis*], from the first references to it as the city is founded in Book 2 to the natural decline of states which begins in Book 8. Rankin (1964, pp. 54–55), though, while recognizing this relationship and tying it to Plato's concern with what exists by nature (i.e., the forms), does try to explain away the significance of the animal language by recourse to "Plato the poet" who was "capable of being led further by the spell of his images than was convenient for Plato the philosopher." Instead of taking the animal analogies seriously, Rankin (1964, p. 92) claims that it "colours the material . . . it is intended to support, and in a sense, perverts it." Adam (1902, Vol. 1, p. 299) is not so ready to discard the appearance of these analogies. In his notes he makes frequent reference to their meaning and significance and recognizes in them "an excellent example of the uncompromising rationalism with which Plato carries out his theories to their logical conclusions."

Adam, however, appears to believe the frequent Socratic refrain that the argument shall determine the direction of the discourse. The argument does no such thing. It is Plato the philosopher and the artist who determines the direction of the argument, who introduces myth or "uncompromising rationalism." We cannot explain away the appearance of animals in the *Republic* by making them examples of poetry or argument conquering Plato; we must instead consider them as part of the Platonic arsenal for clarifying the meaning of philosophy for the human being enmeshed in a political world. The images raise for the reader of Plato's works a variety of problems concerning the nature and perfection of the human being, of the uniqueness and the perversion of the individual by the political community. The source of the analogies must be sought in many different places, but the most important remains Plato's unwillingness to accept the political world of becoming. By turning human beings into animals as they participate in politics, Socrates takes away from them their humanity.

On one level we can see this language as a critique of the Aristophanic character who, concerned with bodily functions, frequently becomes little more than animal on stage. Even those such as Lysistrata and Praxagora, whose political aims may appear noble, are motivated primarily by sex. Politics, as traditionally practiced, treated the individual as a body to be fed, clothed, and shod—as in the first city, the true city (372e), of the *Republic*—and protected, as in the second city of Glaucon's pleasures. The Socratic understanding of virtue was based on the notion of the soul. The primary emphasis in earlier moral discourse had been on man as an external and social being, displaying virtue through beauty and ability in battle, through wealth and political success. The good man was the one recognized as the "most effective in assuring the security, stability and well-being of the social unit, in war and in peace" (Adkins, 1972, p. 60; 1970, pp. 74–79). The political unit did not demand attention to the virtue of souls, since its focus remained on the bodies which comprised it and which could, if need be, be killed in order to protect it. The third city of the *Republic* is an attempt to purge politics of its attention to body. In Book 3 Socrates tries to transform politics into education, turning politics away from the concern with body. Initially, Socrates suggests that the education of his warriors include "gymnastic for the body and *mousikê* for the soul" (376e). The former, however, is entirely forgotten except for a short passage in which the details of the gymnastic education are left to the well-educated mind (403d). Rather, the *mousikê* that dominates the warriors' education eliminates all concern with the body and purges the young men of any strong physical desires for food, drink, or sex. Even doctors who tend to the needs of the sick body are eliminated (405a–410a). Originally the polity emerged because of needs, physical needs of the body for food and shelter (369b–c).[31] Once the focus of the discussion changes in Book 3 to education rather than the polity concerned with necessities, the physical needs of at least one group of citizens are ignored and to the greatest degree eliminated.

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[30] When Socrates describes the deterioration of the best city into the tyranny in Book 8, the animal images continue; here, though, the primary image is that of the bee to which the philosopher had previously been equated: 552c–d; 559d; 564b; 564c; 564d; 564e; 565a; 567d. The bee is a very different animal from dogs and birds which are capable of being trained and manipulated, as bees are not.

[31] It is interesting to note that one of the "needs" is shoes (369d), something Socrates did very well without.

However, in Book 5 we are shown that politics cannot abstract from the body in the way that education does, that politics must confront the Hobbesian problem of organizing bodies in motion, even those of the leaders, for procreation and for war. Socrates' attempt to ignore the body is unsuccessful. The fact that he has introduced the city into his discussion of justice means that he is tied down by the city's orientation towards the body. If Socrates had been able to escape the discussion of the actual organization of the city, of the bodies who comprise the city, as he intends to do at the end of Book 4 (445d), he would have avoided his discussion of women, children and communism. But Adeimantus and Polemarchus do not allow him to escape; he cannot avoid the topic, for politics demands attention to the body and, in those sections of the *Republic* in which the city is founded, ignoring the soul. Human beings are tied by their souls to the divine, as the parables of the divided line and the cave so vividly suggest. It is the soul properly educated that is just and that chooses wisely in the apportionment of lives in the myth of Er. But human beings are also tied by their bodies to the animals. Thus, in the treatment of the political organization of bodies, the animal (and comic) side of humanity predominates. The guardians are bred to produce the finest stock, while all the elements of human sexuality such as enchantment and the shame leading to the desire for privacy are forgotten. In elaborating the social, military, and political structure (as opposed to the educational system) of his Callipolis, Socrates puts the human body into its proper relation to another human body, but omits what he himself recognizes as the defining characteristic of humanity, namely the soul. It is in terms of the soul, what is unseen, that the individual can achieve virtue. We have here once again the contrast between the wrapping and what is inside, the beautiful name and the ugly, comic interior, or the bronze horse and the magical yet dangerous ring of Gyges' ancestor. In structuring his city in Book 5, Socrates concentrates only on the external body, and treating his inhabitants as animals has removed from them the opportunity to achieve their own virtue. In Book 3 the education of the soul for the sake of the polity means training only to be courageous, to hate and harm one's enemies, and to control the passions relating to one's bodily needs. The virtues defined in Book 4 with regard to the war-oriented society before the arrival of the philosopher are those which require submission to the demands of the political community.

By depriving his animals of souls in Book 5 or making the soul serve the needs of an entire polity which is oriented to the body, Socrates takes away from his inhabitants the potential for moral virtue. While the whole regime may be just (though even this may be questioned), there is no opportunity within the structure presented for the individual to be just. The individual, before the introduction of philosophy and eros at the end of Book 5, remains an animal controlled and manipulated by those outside the system. The primary concern of the *Republic* is individual justice. The question posed to Socrates at the beginning is whether justice pays—that is, whether it is more to the individual's interest to be just than to use the ring of Gyges for private power and aggrandizement. The question is which—justice or injustice—serves the individual most. The city demands of its citizens an unselfish virtue and in so doing removes from them their private concern with their own souls—the locus of private virtue. The polis must thus ultimately turn those who will not be philosopher kings into soulless bodies who are controlled much as animals in the barnyard are. The just individual whose soul is properly structured and therefore who is happy needs to escape from the polity, not become immersed in it so as to become little more than a dog or a bird. Even the philosopher kings who are capable of being happy are forced by the city into a life which for them means death (516d; 386c). The tension between private and public builds as the analogy between the individual and polis falls. Socrates finally finds fulfillment away from the animal inhabitants of the best city in the freedom of democracy (557d). In the *Republic* Socrates is not interested in social justice. This is neither the question asked nor the answer given.

Two other dialogues may hint at the significance of animals in the *Republic.* In the *Statesman* a young Socrates listens to a stranger search for a definition of the art of statesmanship. Initially the stranger defines the statesman as one who cares for a herd of two-legged, land-living, hornless animals. In the last analysis, this animal is only a two-legged pig or a featherless chicken. This object of the statesman's control is like the inhabitant of Socrates' ideal city, having no individual soul. The stranger proceeds in his search for the statesman by telling the myth of the age of Cronos. It was an age when God controlled the movement of the earth. Consequently, it was an age of perfection. Demigods were set over the herd of humanity (271d) and, as in the *Republic*, human beings were earth-born (271a–b). Similarly, there was no private property (272a–b), though in the *Statesman* because of a natural abundance rather than the workers of the *Republic*, and there was no possession of wives or children (271e–272a). This age of perfection under the benevolent rule of Cronos is a restatement of the utopia of the *Republic*. But, if we look at the inhabitants of this world we find again that they are only animals, governed by a divine shepherd. The stranger in the *Statesman* goes one step further, people here even talk to the animals (272b); there is no distinction between them. Human beings in the myth of Cronos equal animals because of their perfection, their completion, the absence of any deficiency. In his discourse on the nature of love or eros in the *Symposium*, Socrates defined love as the desire of the good which we are lacking. In the age of Cronos mankind lacks nothing; therefore there is no eros. Throughout the *Republic* there is a similar purging of eros, a deadening of the aspirations. Dramatically it starts immediately with the old Cephalus who can no longer enjoy sex, in which he had indulged as a youth (329c–d), but it receives its full expression in the regime of the *Republic* where there is no love even of one's own body. Thus, the communism and community of wives and children can be accepted. With eros absent from the inhabitants of Socrates' city, its men and women have no potentiality. It is only the philosopher, introduced after the city has been founded in Book 5, who brings eros back into the discussion (474d–475c). But this is only the initial definition of the philosopher. The philosopher who is made part of the city of the *Republic* also lacks this eros. Thus, compulsion enters the city.[32]

The stranger in the *Statesman* continues his myth: after an appointed period of time, Cronos releases the reins of power and the universe unwinding enters the age of Zeus when God no longer controls the movement of the earth. A degeneration away from perfection occurs. Human beings are no longer governed by divine shepherds; they must form their own political communities. Deficiencies appear and with these deficiencies eros returns. There is birth and genesis; it is an age of growth and decay and specifically each individual cares about conception, procreation and the rearing of the young (274a). The communism of the *Republic* disappears and with it the equation between people and animals as well. Human beings, unlike animals, have potential; they desire the good and the beautiful. The stranger had raised the question whether human beings philosophized in the Age of Cronos, but he did not answer the question. The implication is that they did not, for philosophy is an activity of the soul, an erotic activity of striving for that in which mankind is deficient. The perfection of the Age of Cronos and likewise the regime of the *Republic* would preclude such activity. Thus, in the *Republic* the philosopher must be dragged up out of the cave by the founders Socrates and Glaucon. He does not willingly ascend to the light of the sun, and once he has viewed the Good or the sun, the philosopher having reached perfection must be forced back down into the cave. The eroticism of the *Symposium* which drives human beings up the ladder of love in Diotima's imagery is totally absent in the *Republic*. It is only when human beings are allowed to recognize their deficiencies that philosophy is possible. Such opportunities are absent in the perfection of Socrates' utopia, precisely because by creating the perfect city it eliminates potentiality and makes people into animals. It denies them their selfish pursuit of virtue. Consequently, Callipolis fails as an ideal and as the Platonic model for political life.

If we are to look for Plato's "political philosophy" we must look elsewhere than in the *Republic*, or even the *Statesman*. Perhaps the most powerful statement appears in the *Gorgias* where after a long debate on the nature and value of rhetoric, Socrates makes the bold claim that he is one of very few Athenians, if not the only one, to pursue the genuine political craft [*politiké*] and that he is the only man living to put it into practice (521d). According to this notion, the political exists not in the organization of a regime, but in making humanity better. The philosopher king organizing and governing the regime of the *Republic* cannot make people better if his subjects are soulless animals. For Socrates, it appears, the true pursuit of politics must be practiced outside any political organization, whether that of the *Republic* or that of Athens. True political activity occurs not in the highly organized communistic utopia founded in Cephalus' house, but in the private discourse of a few individuals engaged in intellectual inquiry and philosophic endeavor, recognizing their deficiencies, their distance from perfection, as the animals of Socrates' city reenacting the comedies of Aristophanes do not and cannot.

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[32] The parable of the cave which is supposed to represent the ascent and descent of the philosopher ruler is filled with words suggesting the use of force and compulsion. The philosophic dog must be dragged up to see the sun. E.g., 515c,d,e (both *anagnazein* and *bia* are used here); 519c,e; 520a,d; 521b.

The search for justice which initiates the founding of Callipolis must needs go beyond the city, for there is no justice in the city. Thus, the quest continues well after the city has been founded and after it decays, for the city has not provided the answer which Socrates initially expected it would. Instead, it has raised new questions which cannot be ignored in the search for why anyone should live the just life; in its turn, Callipolis presents a further difficulty which must be resolved before we can answer even the first question concerning justice: what precisely is the relationship between the human and animal forms, and what effect does the answer to this question have on the meaning of justice and the purpose of the political community?

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Selecting Cases for Supreme Court Review: An Underdog Model*

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In making review decisions, Supreme Court justices are predisposed to support underdogs and upperdogs disproportionately but, also, are motivated to hide any "bias" that may be at work in determining votes.

In balancing these two values, justices may be expected to vote their "bias" more frequently (1) when that vote will determine outcome, and (2) when the "bias" will be harder to detect. The latter goal may be served by voting the "bias" more frequently in close cases and less frequently otherwise.

In an analysis of the voting patterns of five justices in the decade 1947–56, I found that two liberal and two conservative justices conformed to these expectations. A fifth, or control justice, defined as neither liberal nor conservative, did not pattern his votes in the manner predicted for liberals and conservatives. This relationship held when four projected intervening variables were controlled individually and collectively.

One who attempts to understand the role of the Supreme Court in American life would have very strong incentives to focus on the formal decisions of that Court and the written opinions that accompany such decisions. Throughout its existence, the Court has served up a steady flow of formal decisions on the issues of the day. Many of these decisions have shaped if not shaken the structures and rules that give meaning to social, economic, and political life in the United States. Nevertheless, the justifying premise of this article is that such a focus is too parochial (Ulmer, 1979). It does not take into consideration any number of other decision-making activities in which the Court is constantly engaged. Some of this "other activity" may be of greater significance than formal or plenary decision making. If this is not true generally, it is certainly true in specific instances.

It is my belief that an understanding of the Court's impact on our political system requires that we enlarge the picture to include—at the least—the behavior of the Court in choosing cases for plenary review. Essentially, this is the behavior of the Court in exercising its certiorari and appeals jurisdiction.

If one accepts the need for greater understanding of review decisions, then one must concede the existence of a problem, since our knowledge of the subject is sparse and the Court is absolutely uncooperative when it comes to explaining its reasons for granting and denying review. Some attempts to improve our understanding of jurisdictional authority and how it is exercised have been made, and I have reviewed and evaluated this research elsewhere (Ulmer, 1976). Suffice it to say here that our knowledge of decisional processes at the access or "gatekeeping" stage of Supreme Court decision making is in no way comparable to our comprehension of Court behavior in fully reviewed cases.

In this article, I shall attempt to add something to this bleak picture by investigating the propensity of certain justices to favor or disfavor sociopolitical underdogs.

The Underdog Model

The concept of the sociopolitical "underdog" as a factor in Supreme Court decision making was first broached by C. Herman Pritchett in the mid 1950s.[1] But the most systematic study of litigant status as a predictor of Supreme Court decisions was carried out by Eloise Snyder (1956).

In a study of all Supreme Court cases involving an amendment to the Constitution in the period 1921–53, Snyder found that litigants having "superior" social status or power received a far greater percentage of favorable opinions from the Court than litigants having "inferior" status or power. The actual per-

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[1] The terms "Upperdog" and "Underdogs" are used in C. Herman Pritchett (1954), *Civil Liberties and the Vinson Court.* Chicago: University of Chicago Press.

centages reported are 64 percent and 36 percent, respectively. The status hierarchy used by Snyder was specified prior to analysis and in order of status ran from the federal government and its agents, to state government and its agents, to local government and its agents, to corporations, to labor unions, and finally ended with a category which included employees, individuals, minority group members, aliens, and criminals. The model to be developed here makes use of the Snyder prototype but dichotomizes it with governments and corporations placed in one category and unions, employees, etc. placed in the other. These two classes of litigants may be conceptualized as "underdogs" and "upperdogs" with government and corporations (upperdogs) viewed as having greater status/power than labor unions, employees, minority group members, individuals, aliens and criminals (underdogs). Using these concepts, I propose a modified litigant-status model. The model may be summarized via the following propositions:

1. *In deciding civil liberty cases, Supreme Court justices differ in their predisposition to support underdogs or upperdogs.* This proposition is derived from the observation that on civil liberty scales of plenary cases pitting certain underdogs and upperdogs, the justices do in fact scale.

2. *Some Supreme Court justices are more predisposed to support underdogs than upperdogs, and vice versa.* Proposition 2 merely recognizes what Guttmann scales of civil liberty cases have told us for some time—namely that some justices are consistently toward the top of such scales (high support for under or upperdogs) and some consistently toward the bottom.[2]

Definitions: (a) a justice who consistently supports underdogs over upperdogs in fully reviewed cases is defined as a liberal; (b) a justice who consistently supports upperdogs over underdogs in fully reviewed cases is defined as a conservative.

3. *In making decisions to grant or deny formal review, liberal justices are predisposed to support underdogs over upperdogs while conservative justices are predisposed in the opposite direction.* This proposition is based upon general knowledge about attitude structure. The literature suggests that liberal and conservative voting patterns are a function of the justices' attitudes toward the deprivations imposed on underdogs and upperdogs respectively. Questions of deprivation that tap these attitudinal dimensions stimulate the response patterns observed in fully reviewed cases. We have no theoretical reason for believing that items tapping the same dimensions in other decision-making environments will stimulate other than the same responses. Thus, in changing decision-making contexts, we do not change our prediction of the behavior expected of liberal and conservative justices respectively (Ulmer, 1973).

4. *The tendency of liberal and conservative justices to vote a predisposition is tempered by the felt necessity to be unbiased or impartial* (Goldman, 1976; Sheldon, 1974; Glick, 1970). The justices' individual opinions often pay obeisance to the rule of impartiality as does the slogan embedded in marble over the entrance to the Supreme Court building. That all justices subscribe to the concept of unbiased decision making is reflected in the fact that none is on record as supporting a violation of the principle.

5. *A justice who violates the obligation to provide impartial decision making will seek to* appear *to act impartially.* This follows from the fact that sanctions for violation of role prescriptions flow from the perceptions of others rather than from reality. Since, at the individual level, the two cannot be separated, justices who succeed in convincing their colleagues that they are unbiased may safely proceed in as partial a fashion as they choose as long as the fiction of nonbias retains credibility.

Unlike the votes cast in fully reviewed cases, those cast in review decisions are not made public. However, the appropriate audience for "appearance" at the review decision stage is composed of the voter's colleagues in the Court. Thus, each voter is observed by eight other justices who, over time, may infer bias or nonbias on the part of the voting justice, as appropriate. Indeed, such inferences are more germane at the "gatekeeping" level since at that point a justice is not required to provide any justification for a vote.

6. *Justices appear to act in a less biased fashion when they are in agreement with a larger number of justices than when voting with a smaller number of their colleagues.* In general, objectivity is more easily imputed to majorities than to minorities, to large coalitions than to small ones.[3] This is most obvious in 8–1 splits.

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[2] The traditional civil liberty scale pits government (federal, state, local) primarily against individual civil liberty claimants and is, therefore, narrower than the conceptualization in this article.

[3] I recognize that, stated in absolute terms, such a proposition is debatable at the least. On the other hand, if one were asked to identify the relatively

In such cases, it is easier to think the dissenter biased than to attribute such characterization to the eight-member majority. For, if that were not so, the dissenter might be expected to see requirements of law in the same light as eight colleagues. This is not to say that all dissenters are biased. But, relatively speaking, the *likelihood* of attribution seems greater for dissenters. As the gap narrows, the ease of bias imputation to the minority diminishes. One cannot cite empirical evidence from the judicial literature on this point, but the logical thrust of the argument is compelling. In addition—by definition—if majorities and minorities think each other biased, the larger number of justices making the attribution are on the majority side. The fact of attribution by the larger number surely is not lost on those who find themselves members of small coalitions or single dissenters.

7. *Justices will vote their bias in the open when positive values associated with voting their preferred position are sufficient to compensate for the negative values associated with revealing their bias.* This is derived from research suggesting the validity of compensatory models in explaining Supreme Court decisions in plenary cases (Schubert, 1965, 1974).

8. *Sufficient compensation for revelation of bias exists when a justice's preferred litigant can be assured thereby of having his or her request for review honored or when bias can be obscured by voting with a winning coalition or with a marginally losing coalition.* This is supported in the literature for a single justice—Hugo L. Black (Ulmer, 1977).

9. *In making review decisions, justices who are predisposed to act in a biased manner toward underdogs or upperdogs will attempt to obscure that bias by following three related strategies:* (a) Always voting the bias if vote will determine outcome. (b) Always voting the bias in losing coalitions if the margin of victory for the winning coalition is less than two votes. (c) Otherwise, always voting with the winning coalition with vote undifferentiated by partiality.

This is a more detailed version of proposition 8 and provides a set of testable hypotheses for purposes of evaluating the framework suggested. In general, this proposition is supported in the literature for Hugo L. Black (Ulmer, 1977).

We do not deny that Supreme Court justices may be responding to factors other than those we have described in making their decisions to grant or deny plenary review. It is certainly possible that justices will vote to deny review because they think the chances of acceptable decision on the merits unobtainable, because of interaction with other justices whose judgment they respect, because they believe the judgment made in the lower court is correct, because they think the issue raised is trivial, or for any number of other reasons. We are not concerned here, however, with all theoretical possibilities. We seek to determine the extent to which a restricted model will contribute to explaining variation in voting patterns in certain selected situations. This inquiry is encouraged by the fact that no previous work has been able to explain more than 30 percent of the variation observed in the grant-deny patterns. The level of theory sought is partial rather than general. But given the paucity of knowledge in this area, derivation of singular generalizations would appear sufficient to justify the effort involved.

Operationalizing the Model. A vote cast by a single justice in making review decisions is cast in the context of the justice's estimate of nine different voting configurations ranging from 0–8 to 8–0. The only case in which the justice's vote will determine outcome, however, occurs when the vote to grant review is exactly 3.[4] This follows from the procedure by which such decisions are made in the Court. If as many as four justices wish to accept a case for which review is requested, the request is granted; otherwise, it is rejected. Given the model assumptions, liberal and conservative justices would be expected to exhibit kurtosis strongly favorable to their preferred litigant class in 3–5 situations. In addition, the model asserts that, on balance, the utility of voting predispositions may be sufficient to overcome the costs of revealing bias if the vote can be "objectified" by casting it in the context of a relatively large coalition; i.e., the justice does not wish to vote bias alone or in a group so small as to imply bias. This second consideration adds to the 3–5 splits all cases in which a justice is faced with 4–4, 5–3, and 2–6 configurations. We shall label this group of

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baised members of the Court and had nothing beyond voting patterns to go on, one would be well advised to choose as more biased those who dissented frequently in small groups. Certainly this would be indicated in respect to biases that tend to be idiosyncratic for particular justices. A prime example here, one might argue, is William O. Douglas who not only voted his values/biases on a wholesale basis but also made few bones about it.

[4] All voting splits referred to in this paper will follow the convention of listing the vote to grant review first.

cases set 1. The remaining cases, i.e., those in which the justice is faced with 8–0, 0–8, 7–1, 1–7, and 6–2 splits, we classify as set 2.[5] The model leads us to expect that liberals and conservatives will exhibit voting patterns in set 1 cases more consistent with the bias attributed to them than the patterns revealed in set 2 cases. This follows from the fact that in set 2 cases, to vote bias in half of the theoretically possible voting configurations would require alignment with coalitions losing by 2 or more votes, thereby violating proposition 9(b).

The Burton papers provide the data to be used in testing the model. Burton recorded the votes of the individual justices on all certiorari applications considered in conference in the 1945–58 terms. Focusing on the 1947–56 terms, our sample consisted of all the cases in the 1951 and 1955 terms and one third of all remaining cases.[6] From this sample I isolated all cases in which upperdogs and underdogs were opposing litigants at the level of the review decision.

Within the same decade, I identified the justices who served throughout the period. By this criterion, I selected five justices–Douglas, Black, Frankfurter, Burton, and Reed. By the definitions given earlier, the first two were classified as liberals, the last two as conservatives, and the middle justice as "control." I was guided in making these classifications by the mean ranks of each justice on civil liberty scales across the decade.[7] These means were 1.4 for Douglas, 2.4 for Black, 3.7 for Frankfurter, 6.9 for Burton, and 8.5 for Reed.[8]

The control justice is viewed as neither liberal nor conservative. The model, therefore, should not predict that justice's behavior. However, if neither liberal nor conservative, the control justice should not behave as either is expected to do. If we observe the control justice behaving as specified for liberals or conservatives, then some finer tuning of the model would be in order.

Data Analysis and Findings. Our initial focus is on the voting patterns of the liberal and conservative justices in making review decisions. The order in which the votes are cast is not known with certainty, but it could be the same as that used when voting on the merits of a case–i.e., reverse seniority. In any event, that order is not significant for our purposes since our model implies that each justice must estimate the final voting order in advance in order to make a model-rational choice.

This consideration ties into a feature of the Burton data. Burton recorded only the votes of the justices on review petitions which came to conference for collective discussion and individual voting. During the period covered by the data in our sample, approximately two-thirds of the applications for review were denied by so-called special listing procedures. Under these procedures, the Chief Justice (with Clerk assistance) screened all applications for "triviality." Petitions considered trivial were placed on a list which was circulated among the Associate Justices. Any case on the list could be removed for consideration at a conference upon the motion of a single justice. Otherwise, all listed petitions were simply denied review without discussion or voting in a conference setting.

Our model requires discussion in a collective setting if justices are to make estimates of voting split. Thus, we would have to limit our data to conference cases only–even if Burton had not done so. The first step in analysis is to see if the liberal and conservative justices voted consistently with their assigned predispositions across all cases. Table 1 arrays the summary data. In the first data breakdown, we find that across all cases, the voting patterns for the liberal and conservative justices are as anticipated. The relationships are statistically significant and the directions are pro-upperdog for Burton and Reed but pro-underdog for Black and Douglas. Also, as expected, the control justice, Frankfurter, did not distinguish upperdogs and underdogs to a statistically significant degree.

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[5] It may be observed that 2–6 and 6–2 splits are not two sides of the same coin. A justice joining two colleagues in the first instance loses by a one-vote margin. A justice joining two colleagues in the second case loses by a three-vote margin. This is a consequence of granting review when four of nine justices so desire.

[6] This sample has been discussed at greater length in S. Sidney Ulmer, "The Decision to Grant Certiorari as an Indicator to Decision on the Merits," *Polity* 4:439–47. It has recently (in March 1978) been forwarded to the Inter-University Consortium for Political and Social Research at Ann Arbor and, after appropriate processing, will be available for general distribution.

[7] The scales used are reported in Glendon A. Schubert (1965, pp. 104–09). Civil liberty scales were used because liberal and conservative justices in the Court are usually classified by their propensities in civil liberty cases. Moreover, scaling in the judicial subfield has been applied most frequently to that category of cases and the inferences from such scales attract a greater degree of confidence than when drawn from other case areas. An alternative method of classifying justices would be to use both C and E (Economic Enterprise) scales. If that is done, Frankfurter is closer to the conservative than to the liberal position. However, the ranking of the justices is purely a relative matter and using either C or C + E scales, Frankfurter comes out in the center between Black and Douglas on the one hand and Burton and Reed on the other.

[8] Reed did not quite complete the tenth year but has been used anyway.

The gammas (Freeman, 1965) in the table measure the probability that knowing whether the petitioner is upperdog or underdog, we can predict the decision of the justice to grant or deny review. The associations between the two variables are highest for Black and Douglas and lowest for Reed and Burton. Our main concern, however, is not so much with the overall relationships but with the comparable gammas across the two sets of cases specified in the model. According to the model, set 1 cases are favorable for revelation of bias while set 2 cases represent unfavorable conditions for real or apparent partiality. To be consistent with the model predictions, the gammas for the liberals and conservatives across both sets should be in the same directions as in Table 1 but higher in the favorable condition and lower in the unfavorable condition.

Table 2 shows that for the liberals and conservatives, the association between the variables is in each case weaker in the set 2 condition than under set 1. Comparing the two sets, the gammas are considerably higher in the first for Black, Douglas, and Burton and moderately higher for Reed. The control justice—Frankfurter—is the only justice to reverse direction and therefore is unlike either liberals or conservatives. As it turns out, the effective impact of Douglas (and Black) in assisting underdogs is considerably greater than Frankfurter's even though the differences in the voting behavior in the two sets is greater for Frankfurter than for Douglas.

The revelation of hypothesized relationships in a data set is merely the first step in analysis—though often a difficult one. Correlational analysis cannot prove that the connection between litigant status of a petitioner for Supreme Court review and decision to review is necessary. We know however, that these factors occur in a time sequence and we have shown that they vary together. We have also shown that the extent of covariation is affected by certain conditions. This second step in our

Table 1. Analysis of Votes of Five Supreme Court Justices to Grant or Deny Review, 1947–56 Terms, in Cases in Which Upperdogs and Underdogs Were Opposing Litigants

Justice	Cell Entries*	$p(x^2)$	Gamma
Reed	(109,218,125,779)	.001	.514
Burton	(92,227,151,817)	.001	.373
Frankfurter	(72,264,168,772)	.176	.112
Douglas	(78,495,153,506)	.001	–.314
Black	(64,484,176,549)	.001	–.416

*Numbers in this column refer to entries in the traditional a, b, c, d cells of a fourfold table. For example, in the case of Justice Reed, the four cell table entries would be 109 in cell a, 218 in cell b, 125 in cell c, and 779 in cell d. (a) is for review granted when upperdog is petitioner; (b) for review granted when underdog is petitioner; (c) for review denied when upperdog is petitioner; and (d) for review denied when underdog is petitioner.

Table 2. Analysis of Votes of Five Supreme Court Justices to Grant or Deny Review in 2–6, 3–5, 4–4, 5–3 Cases (Set 1) and 8–0, 0–8, 6–2, 7–1, 1–7 Cases (Set 2)

	Reed	Burton	Frankfurter	Douglas	Black
Ns					
Set 1	(36,74) (30,246)	(36,74) (39,268)	(15,108) (55,228)	(25,210) (40,68)	(12,212) (50,67)
Set 2	(72,144) (95,533)	(55,153) (112,549)	(56,156) (113,544)	(52,285) (113,438)	(52,272) (126,482)
Gammas					
Set 1	.599	.539	–.269	–.663	–.859
Set 2	.474	.276	.266	–.174	–.155
Difference	.125	.263	.535	.489	.704

Note: Ns refer to cell entries for the four cell tables displayed in a,b,c,d manner. For Reed, a=36, b=74, c=30, d=246, and so on.

analysis, in effect, controlled for a specific context to see if the context of covariation made a difference. We found that it did. Since our logical model has provided a reasonable explanation for this phenomenon, one might be willing to make the intellectual leap to a causal interpretation. To do so would be premature.

The differences in the levels of observed covariation could possibly be accounted for by factors exogenous to our model. Had we been able to choose cases identical in every respect except petitioning party, this problem would not occur. But taking cases as they come, there are factors that might covary to an equal or greater extent with the decision to grant or deny review. If these factors can be identified, we may ask whether the result we have reported is maintained when each is held constant—i.e., not allowed to vary.

We have identified four factors for this phase of the analysis; whether there is dissension in the lower courts regarding the case,[9] whether the case raises a civil liberty question, whether the case raises an economic question, and the level of the court from which the appeal was taken. The first three of these were incorporated in the Cue Theory model developed by Tanenhaus and his associates earlier (Tanenhaus, 1963).[10] The fourth was chosen on the premise that a justice might be influenced by the level of court in which the last decision was made. Here we have differentiated among state courts, federal district courts, and federal courts of appeals. The other three variables have been dichotomized in present-absent terms.

We wish to know whether the observed disparities in covariation between two sets of cases can be accounted for by variation in any one of these four control variables. The form of the control we employ is statistical—the partial gamma (Kirkpatrick, 1974). In linear regression, a squared partial correlation coefficient is defined as the percentage of variation that can be explained by an individual variable after the control variable(s) has accounted for all the variation it can (Blalock, 1960). In the present case, similarly, the partial gamma represents the predictive capacity of litigant status remaining after a control variable has been allowed to exhaust its predictive power.

Since we are dealing with a single control variable each time, first order partial gammas are required. Classifying cases by each control variable—one set at a time—we have computed the zero order gamma and the first order partial gamma.[11] In each of our four categories, the zero order gamma measures the probability of prediction ignoring the variation in the control variable. By comparing the zero and first order partial gammas, we can see how the control variable affects the relationship between litigant status and review decision. If the two coefficients are the same, then the control variable

Table 3. Analysis of Votes of Five Supreme Court Justices to Grant or Deny Review in Set 1 and Set 2 Cases, Controlling for Four Conditions One at a Time

	Dissension		Civil Liberty		Economic		Court Level	
Justice	Set 1	Set 2	Set 1	Set 2	Set 1	Set 2	Set 1	Set 2
Reed	.577	.466	.587	.443	.577	.430	.578	.473
	.578	.378	.528	.430	.550	.431	.552	.447
Burton	.545	.245	.549	.237	.541	.219	.530	.295
	.530	.127	.554	.216	.576	.247	.582	.273
Frankfurter	–.268	.243	–.285	.231	–.291	.214	–.266	.265
	–.255	.141	–.159	.262	–.231	.250	–.354	.241
Douglas	–.643	–.187	–.649	–.202	–.647	–.209	–.676	–.198
	–.645	–.232	–.634	–.155	–.637	–.180	–.610	–.179
Black	–.852	–.175	–.857	–.191	–.854	–.197	–.858	–.180
	–.847	–.208	–.853	–.158	–.851	–.209	–.873	–.170

Note: Zero order gammas are given first with the relevant first order partial gammas immediately below.

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[9] Defined as disagreement among judges in the Court immediately below or between courts and agencies involved in the case below.

[10] It should be noted that Tanenhaus used all decisions, not just conference decisions. Therefore, the failure of his cues to explain variation in conference decisions will in no way contradict his findings. Elsewhere, I have suggested a two-stage conceptualization of the review decision process (Ulmer, et al., 1972).

[11] The zero order gamma for each set of cases is not necessarily the same as the zero order gamma for all cases since the control variable may cause the Ns to vary.

has no impact on the association. If the partial gamma is zero, the control variable accounts for the association entirely. For purposes of testing our model further, we are interested in knowing whether the differences observed in the gammas for the two sets of cases initially are maintained when partial gammas are computed. Table 3 summarizes the results of this analysis.

Looking at the table, we see that whether we are comparing zero and partial gammas in set 1 or in set 2, the control variable generally has no effect on the original relationships. Out of 40 such comparisons, only 4 suggest any effect for control variables. Three of these are accounted for by our control justice—Frankfurter. When dissension is controlled in set 2, the gamma drops from .243 to .141, suggesting that Frankfurter was influenced somewhat by the dissension variable. A similar drop is observed for Frankfurter when civil liberty question is controlled in set 1. But when court type is controlled in set 1 cases, the association for Frankfurter between litigant status and review decision is enhanced. Since these discrepancies are for our control justice, they do not in any way distract from the case for our model. Indeed, the model implies that the Frankfurter behavior will be discrepant with that of the liberal and conservative justices.

The only other instance of control variable effect is in set 2 cases for Burton controlling for dissension. In such cases Burton exhibits a gamma drop from .245 to .127. Eliminating Frankfurter, we can say that of 32 comparisons, a control variable has an appreciable impact in only one. It logically follows that the observed differences in the covariation levels of litigant status and review decision between two sets of cases will be maintained when controls are instituted. That is precisely what Table 3 reveals. In a majority of the cases, the magnitude of the difference increases when a control is imposed. In the remaining cases, without exception, the approximate magnitude of the difference is observed. Thus—putting all this together—our confidence in our model explanation remains intact.

As a final step, we ask whether the four control variables collectively have an impact on the ability of voting split to influence predispositional voting. A partial gamma analysis is also useful in answering this question. Table 4 presents the fourth order partials between petitioning party and voting behavior for two sets of cases controlling for dissension, civil liberty question, economic question, and court level. Once again, we see that the liberal and conservative justices maintain their directional preferences and Frankfurter his reverses of direction. However, the fourth order partials for Frankfurter are not of great magnitude in either set 1 or set 2 cases. That is, as expected, Frankfurter's behavior differs from that of the liberal and conservative justices.

For liberals and conservatives, there is a drop in the partial coefficients in five of eight comparisons under control conditions. However, in all five the decrease is minor. In the cases of Douglas, Burton, and Black, there is a slight increase in the partial coefficients. Yet, in all eight cases, it is obvious that the control variables collectively do not affect, in any significant way, the basic association between voting choice and petitioning party. Most importantly, for purposes of testing model predictions, the disparities between the coefficients for set 1 and set 2 cases mirror in every instance the differences revealed in the earlier analyses. The largest difference is associated with Justice Black, with Burton, Douglas, and Reed, following in that order. This order is similar to that observed in comparing first order gammas under four conditions in Table 3 and the zero order gammas in Table 2.

These disparities for Black set him off, perhaps, from the other justices since they are so large. This is due, possibly, to a combination of factors—his intense feeling for underdogs stemming from his earlier populist leanings—and a firmer commitment to the role prescrip-

Table 4. Partial Gamma Analysis of Votes of Five Supreme Court Justices to Grant or Deny Review in Set 1 and Set 2 Cases Controlling for Four Conditions Simultaneously

	Set 1		Set 2	
Justice	G12	G12.3456	G12	G12.3456
Reed	.550	.480	.433	.350
Burton	.550	.608	.242	.117
Frankfurter	–.282	–.083	.219	.162
Douglas	–.658	–.627	–.236	–.254
Black	–.856	–.883	–.213	–.174

Note: For this analysis, the variables are (1) vote, (2) petitioning party, (3) dissension, (4) civil liberty question, (5) economic question, and (6) court level.

tion of impartial judging than that possessed by the other justices (Pritchett, 1948; Frank, 1969). While this is speculative, it is consistent with Black's behavior. A justice with strong feelings on both these dimensions would be expected to vote his predispositions frequently but to go to greater lengths to obscure the meaning of such voting acts—as Black appears to have done.

Summary and Conclusions

In this article we have suggested the importance of the Supreme Court's review decisions for understanding the American political/judicial systems and the theories underlying them.[12] We have sought to add to our limited knowledge about these processes by examining voting on certiorari applications and certain specific conditions under which that voting occurs. The main focus has been on the effect of voting split and petitioning parties on voting behavior. Given knowledge of these conditions, I have developed a systematic model to predict voting patterns for five Supreme Court justices serving across the decade covered by the 1947–56 terms. The model is theory-integrating in that it combines propositions from attitudinal and role theories in a logically coherent fashion. In general it posits that liberal and conservative justices are predisposed in review decisions to vote for underdogs and upperdogs, respectively, but that a willingness to act on such predispositions will be tempered by the role prescription of impartiality. The model suggests this conflict will be resolved to some extent via the *appearance* of impartiality while actually voting according to a bias.

Using data from the Harold Burton papers, I have tested model predictions and found them reliable in every case. I infer that liberal and conservative Supreme Court justices (as I have defined them) are motivated to vote their predispositions toward litigants seeking review but also to minimize the costs of such acts by nesting them in conditions under which their significance is not fully appreciated. This conclusion must be viewed, however, in proper perspective. I have examined only five justices across a single decade. It is possible to extend the analysis across another ten or eleven justices for about the same time span. But to improve on these numbers will require new data of the sort I have examined.

It is also possible that my data configurations are subject to explanation by other models or by some modified version of the one presented. While I have not examined all possible variables that might affect the relationships I have portrayed, the four chosen for investigation appear to be the most obvious candidates for consideration. One cannot rule out the possibility, however, that variables excluded from the model possess explanatory power.

Assuming that the model presented has some validity, and given that the role prescription of impartiality permeates the political, legal and governmental systems of the United States, research designed to identify and measure response to such a consideration throughout official decision-making agencies should be encouraged by the results we have reported for Supreme Court review decisions. We may wish to know whether the justices behave similarly in deciding fully reviewed cases, but also whether other judges in other courts and decision makers in nonjudicial arenas reveal like tendencies in the decisional settings in which they operate.

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[12] Lawrence Baum (1977) has written an interesting paper suggesting the importance of this question at the state level.

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Judges' Role Orientations, Attitudes, and Decisions: An Interactive Model*

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Despite almost two decades of behavioral research, our models relating the key variables in judicial decision making are incomplete and inadequate. In particular, the impact of two widely used variables, judges' attitudes and role orientations, is poorly understood. While there appears to be a consensus that attitudes and role orientations are important predictors of behavior, no research has been successful in developing a comprehensive model capable of predicting judges' behaviors. This article's objective is the development of a single model incorporating attitudes, role orientations, and decision-making behavior. While attitudes and role orientations taken singly explain insignificant amounts of the variation in behavior, an interactive model of attitudes and role orientations is shown to be extremely useful for understanding behavior. Although this research focuses on the sentencing decisions of Iowa trial court judges, the proposed model is potentially applicable to all instances of decision making.

Despite almost two decades of behavioral research on judicial decision making, our models relating the key variables in the process are incomplete and inadequate. In particular, the impact of two widely studied concepts, judges' attitudes and role orientations, is poorly understood. While there appears to be a consensus that attitudes and role orientations are important predictors of behavior, no research has been successful in developing a comprehensive model of the interrelationship which is capable of predicting judges' behavior.

Part of the inability to explain decisions may be related to the highly simplified models of decision making which have been employed. The models are simple and incomplete in two respects. First, they posit direct and unconditional linkages among the variables; that is, they fail to consider the interaction of different variables and the complex way in which they may relate to decisions. The second deficiency is that they are statistically unsophisticated. Much judicial literature is based on relatively unenlightening statistical techniques. Differences of means tests, for instance, indicate very little about the degree to which one variable is dependent upon another.[1] Guttman scaling, the grandfather of judicial statistics, is a relatively unpowerful mode of analysis which can produce questionable results.[2] These problems are especially severe in multivariate analysis. Finally, most statistical analysis has been limited to bivariate relationships, with little emphasis on multivariate modeling.

Thus there is a pressing need to develop a more complete understanding of this complex process. One approach with great potential for providing a fuller understanding is to examine the decision process from a multivariate perspective and to examine the interaction among important independent variables. This strategy may result in an improved ability to predict, or explain, decisions. The research discussed in this article, rather than limiting itself to a single independent variable, focuses on two concepts, attitudes and role orientations, in an effort to explain the sentencing policies of trial court judges in Iowa. Rather than merely reporting bivariate and multivariate results, I have placed particular emphasis on developing a theory of the interaction of role orientations and attitudes, and testing the theory against the data. The utility of the proposed model is strongly supported by the unusually high proportion of variance in the decision-making process which is explained.

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[1] For a good example of this point see Bowen's (1965) reanalysis of Nagel's (1961) work on social backgrounds. Similarly, Hagen (1974) has challenged the conventional wisdom on the effect of the race of defendants in criminal cases on their sentences.

[2] "Like the blood test for paternity, cumulative scaling can eliminate erroneous hypotheses, but it cannot by itself demonstrate the existence of true ones" (Tanenhaus, 1966, p. 1589). See especially pp. 1590–94 for a further critique of Guttman scaling.

Attitudes as Determinants of Judicial Behavior

There is little question that the predominant paradigm of judicial decision making places judges' attitudes in the center of the process. Indeed, it is not an overstatement to assert that attitudinal approaches have become the traditional nontraditional mode of judicial analysis. The basic theoretical framework for the analysis of attitudes and behavior adopted by this research asserts that an attitude represents a residue of previous experience which exercises a patterned tendency to respond to stimuli in a certain way (Campbell, 1963). Attitudes can be thought of as biasing behavior from a straight stimulus-response model. The notion that behavior can be predicted solely from knowledge of external stimuli is explicitly rejected. Attitudes represent generalizations about phenomena based upon extrapolations from previous experience and usually take the form of cognitive generalizations.

External stimuli have vastly different effects depending upon this residue of experience. The stimuli of cases do not impinge upon a tabula rasa. Thus, attitude theory points to the experience of the decision maker as a key variable in the decision equation. What evidence exists to support the theory?

Principal support for the attitude model is provided by studies of decision making in the U.S. Supreme Court.[3] These studies all rely on the judge's votes as the single bit of datum and scaling is the predominant method of quantitative analysis. To the extent that the votes demonstrate consistency, attitudes are postulated as the organizing element. The attitude is never measured—its existence is inferred from consistency in voting behavior. There is no empirical test of the relationship between attitudes and behavior beyond the test of consistency, and the substance of the attitude must be inferred from the substance of the cases. Further, this type of analysis makes it impossible to examine the relationship among attitudes, or any interaction effect of attitudes on behavior (and indeed, any complex model of the relationship). It is impossible to eliminate, on empirical grounds, rival hypotheses (such as hypotheses of small group, or game, influence[4]) which may account for the consistency. Due in part to data limitations, research of this type is limited to a simplistic and incomplete model of attitudes and behavior. The "success" of attitude variables in predicting behavior is apparent only in a somewhat limited sense.

The only way to construct a more comprehensive model of decision behavior is through independent empirical measures of all variables in the model. Some attitude studies have proceeded in this way (Nagel, 1963; Hogarth, 1971). Other research uses disparities in sentencing behavior to infer the existence of an attitudinal influence (Atkinson and Neuman, 1970; Green, 1961).[5] Several other studies have focused on a particular attitude, role orientation, and will be discussed below. Thus, while behavior can be explained by attitudes, at least in part, the degree of influence and the process of influence are as yet unspecified.

Data and Research Setting

This research investigates the decision-making process in the Iowa district court system. The district courts are the state trial courts of general and original jurisdiction, and are typical of many state trial courts. All criminal charges are either felonies or indictable misdemeanors. In noncriminal matters, the district courts try most major civil actions. The system is organized into 99 county courts. There are 8 judicial districts in Iowa, with each district composed of a number of counties. This research focuses upon the 3 southeastern districts of the system. Although the districts were selected primarily on ease of access criteria, they are representative, at least on several aggregate indicators, of the state trial court system. Twenty-five of the 99 counties and 24 of the 83 judges are included in these 3 districts.

The universe of criminal cases analyzed includes all cases initiated during 1972 and 1973 and concluded by the end of 1974. The data were collected from criminal case files stored at each of the county courthouses. In 22 of the counties no sampling was necessary, so the universe of cases was selected. In the remaining 3, the most populous counties in the districts, I used random sampling, selecting 5,350 cases. In 2,715 cases, 50.7 percent of the total, convictions resulted. Since the focus of this study is on sentencing, I excluded dismissals from the analysis.

These case data provide the basic dependent variable: sentencing outcomes. The independent variables are derived from personal interviews, held in early 1975, with the judges who sentenced the defendants in these cases. A total of 27 judges was responsible for the sentences in the criminal cases. Twenty-six of these judges were interviewed (the remaining judge no longer resides in Iowa). The interviews lasted an average of one hour. From these interviews I derived several basic attitudinal measures, as well as several measures of the role orientations of the judges.

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[3] Virtually all behavioral studies of the U.S. Supreme Court can be placed in this category. See, for examples, the works of Pritchett (1941); Spaeth (1963); Schubert (1975); and Ulmer (1960).

[4] On small group influences see Walker (1973). For an application of game theory see Rohde (1972).

[5] Most studies of racial discrimination in sentencing could also be placed in this category.

The dependent variable of concern to this study is the severity of criminal sentences. On its face, sentence severity would seem easy to measure quantitatively. In reality, the difficulties of such measurement are considerable. There are three characteristics which are highly desirable in any measure of sentence severity: (1) severity should be measured at least at the ordinal level; (2) it should be a single scale which makes use of all the different penalties in sentences; and (3) it should be a single scale equally applicable to sentences for crimes of widely varying seriousness. With one exception, a scale for measuring sentence severity first proposed by the Administrative Office of the U.S. Courts and later applied by Beverly Cook and others to sentences in federal criminal cases satisfies these criteria (Cook, 1973; Tiffany, Avichai, and Peters, 1975). The scale is based on (1) the length of probation, (2) the length of incarceration, (3) the amount of fine, and (4) whether the sentence was suspended or whether a deferred sentence was imposed.

While this research is mainly concerned with the impact of attitudes on sentencing behavior, many other factors may have an impact on sentences. If these influences are not controlled for, any observed relationship between attitudes and behavior might be spurious. This problem, one not faced by research on collegial courts, stems from the fact that the judges are deciding different cases and the cases (and defendants) may have different characteristics. If case characteristics are related to sentence severity, then differences in sentencing among judges may not be due to attitudes or role orientations but may instead be due to the fact that they hear different kinds of cases. Consequently, it is prudent to examine the impact of several of these characteristics on sentence severity.

Nine characteristics of the cases and defendants have been identified as potential influences on sentencing decisions. These are: (1) the seriousness of the charge (based on the legislatively defined maximum sentence), (2) whether the defendant was released on bail, (3) the plea of the defendant, (4) whether the defendant's counsel was privately retained, (5) the defendant's sex, (6) whether there were multiple cases against the defendant, (7) the number of previous misdemeanor convictions, (8) the number of previous felony convictions, and (9) the age of the defendant.[6] The Pearson correlations between each of these variables and the Administrative Office severity scale are shown in Table 1. Only two of the variables, charge seriousness and whether the defendant was released on bail, are even moderately related to sentence severity. The type of counsel retained by the defendant and the defendant's plea are weakly related to sentence severity. However, the seriousness of the charge is the only variable which has a significant *independent* impact on sentence severity, as suggested by the insignificant partial correlation coefficients which result when the impact of seriousness is controlled. This of course suggests that the seriousness of a charge determines *both* sentence severity and, for instance, whether the

Table 1. Case Characteristics and Sentence Severity

Case Characteristics	Bivariate Pearson Correlation	N	Partial Correlation Controlling for Charge Seriousness
Charge Seriousness	–.58	2570	–
Pretrial Release on Bail	–.41	1815	–.23
Defendant's Plea	.24	2706	.21
Type of Counsel	.22	2730	.04
Defendant's Sex	–.05	2516	–.06
Number of Cases Against Defendant	.12	2730	.05
Previous Misdemeanor Convictions	.15	564	.07
Previous Felony Convictions	.12	629	.08
Defendant's Age	.13	1347	–.01

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[6] The race of the defendant was also considered but since the number of black or Indian defendants is quite small the variable was omitted due to lack of variance. Social class was not measured but it can be assumed that there is little variance in this variable.

defendant is released on bail prior to trial. Charge seriousness is therefore the only variable for which it is necessary to control.[7]

The (linear) relationship between sentence severity and charge seriousness is fairly strong: a third of the variance in sentence severity can be explained by charge seriousness. However, the total impact of charge seriousness on severity is not linear. Eta^2 is .71, indicating that an additional one-third of the variance in sentence severity can be accounted for through a nonlinear relationship. Indeed, the null hypothesis that the variables are linearly related can be rejected at the .001 level. This suggests that the judges do not react to various crimes in the same way as legislators. More importantly for this analysis, the effect of this variable must be controlled both linearly and nonlinearly. Converting the sentence severity scores to "Z" scores *within each category of charge seriousness* is one way to remove the impact of charge seriousness on sentence severity. Using this procedure, the severity of each sentence is measured only in relationship to crimes of equal seriousness. By placing all of the sentences on a common metric (Z scores) the sentences become comparable. To compute severity scores for judges it is now necessary only to compute the mean standardized severity score for the cases each judge decided. These means are the dependent variables for the attitudinal portion of the analysis.

Findings: Attitudes and Behavior

The first step in this analysis is to determine the strength of the influence of judges' attitudes on their behavior. Since independent measures of both attitudes and behavior are available, we need not limit the research to inferences about consistency in behavior, but rather can directly examine the relationship.

While judges have attitudes on an enormous spectrum of subjects, only a few of these attitudes are potentially relevant to the judicial decision-making process. Previous research has focused upon the concept "political liberalism." "Liberalism" has been defined as "the viewpoint associated with the interests of the lower or less privileged economic or social groups in one's society and (to a lesser extent) with the acceptance of long-run social change" (Nagel, 1963, p. 29). Liberal judges are expected to be less severe in sentencing because of their receptivity to the interests of the "lower or less privileged" groups, the groups from which the overwhelming majority of criminal defendants originate.

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[7] Under some circumstances it might be desirable to remove directly the impact of all of these nine variables on sentence severity. Beyond the question of linearity (which is considered next), this might be accomplished by regressing these sentence severity scores on the nine variables and then computing residuals. There are two reasons why that approach was not selected. First, a substantial quantity of data is missing for some of the variables (see the Ns reported in Table 1), requiring that the regression be performed on a matrix of correlations based on significantly different subsets of the total cases (for a discussion of the problems inherent in pairwise missing data deletion see Cohen and Cohen, 1975, Ch. 7). Even more important is the problem of how missing data are handled in the computation of the residuals which would be used to create the dependent variable for the remainder of this analysis (i.e., the method of computing $\hat{y}$). While a number of alternatives exist (such as computing $\hat{y}$ by scoring missing values as the mean of the variable) there is little statistical justification (beyond expediency) for their use, especially under circumstances such as these.

The second justification for controlling only for the seriousness of the charge concerns the minor independent contributions of the other eight variables. As Table 1 demonstrates, the impact of the eight variables is negligible once the effect of charge seriousness is partialed out. The defendant's age, sex, and prior record, as well as the number of charges filed against the defendant, are virtually independent of sentence severity (they explain less than 3 percent of the variance in severity), so even without partialing it is unnecessary to control for them. Type of counsel is weakly related to sentence severity but type of counsel is related to charge seriousness ($r = -.32$) and it is this relationship which accounts for its impact on sentence severity (as reflected by the partial correlation of .04). Defendants with appointed counsel receive more severe sentences than defendants with private counsel, but that is only because defendants who cannot afford private attorneys tend to be prosecuted for more serious crimes (possibly because middle-class, or white-collar, crimes are rarely prosecuted). However, the impact of the defendant's plea on sentence severity is not affected by the control for charge seriousness but plea is only very weakly related to sentence severity (explaining less than 6 percent of the variance) and only 4 percent of the cases were not decided by guilty pleas. Finally, defendants incarcerated prior to trial receive more severe sentences ($r = -.41$) but that is largely because those incarcerated prior to trial are charged with more serious crimes ($r = .41$).

Thus, if it can be assumed that charge seriousness is logically antecedent to bail and to the type of counsel (and it is at least temporally antecedent), then the impact of the other variables is so slight that there is little need to control for them. This suggests that even if the missing data problem were solved the results of the analysis would not differ appreciably: the residuals and the observed data would be very highly correlated. Given the quantity of missing data and the low correlations, it is preferable to control only for charge seriousness.

In the interviews with the sentencing judges, I used measures of several subdimensions of political liberalism. The subdimensions are: (1) government economic intervention; (2) class-based beliefs; and (3) religious influence on society. The government intervention scale is composed of five Likert items; a scale of five items measures class-based beliefs; and three items are used to form the religious influence scale.[8] I hypothesized that "liberals" are more lenient in their sentencing behavior.

A fourth measure of liberalism was also used. I asked the judges to identify themselves subjectively on a liberalism-conservatism dimension, and coded their responses on a five-point scale (liberal, leaning toward liberal, middle-of-the-road, leaning toward conservative, and conservative).

To test the hypothesis that "liberal" judges are more lenient, I correlated the sentence severity means with the four measures of liberalism, and the results are shown in Table 2. In general, the hypothesis receives no support from the data. Liberals are not in any meaningful sense less severe in sentencing than conservatives. The strongest relationship observed accounts for only 10 percent of sentencing variance and the relationship between self-identification and sentencing is in the wrong direction. The correlations are so consistently weak that one may place considerable faith in the findings.

Political liberalism may be too unfocused to provide the judge with much guidance in sentencing; that is, whether a judge is liberal or not may be largely irrelevant to the sentencing decision. Liberalism, to the extent that it is relevant to sentencing, must be more closely related to the actual operation of the criminal justice system. More specific attitudes, like beliefs about crime and the criminal justice system, may be better predictors of sentencing behavior.

Since I included in the interview measures of the judges' beliefs about crime and justice, it is possible to assess the hypothesis that these more relevant attitudes affect their decisions. Beliefs about crime and the criminal justice system have been measured along several dimensions. The first concerns attitudes toward the causes of criminal behavior. Judges who believe criminal behavior to be the result of social and environmental forces are hypothesized to be more lenient in sentencing than judges who view crime as a moral, free-choice decision. I measured this attitude using a scale of six Likert items. The responses to the items were factor-analyzed using principle components analysis.

The second of the measured criminal justice beliefs is the judges' goals in sentencing. Judges who hold rehabilitative goals and who are concerned for the perpetrator are hypothesized to be more lenient in sentencing than judges with a greater concern for the crime victim and with punishment or deterrence as objectives. This measure uses a scale of eight Likert items. The items were also factor-analyzed, resulting in factor scores.

Table 2. Liberal Attitudes and Sentencing Behavior[a]

Liberalism Subdimensions	Pearson *r*	*b*	Beta
Attitude toward Government Economic Intervention	–.01	.01	.08
Class-Based Beliefs	–.14	–.00	–.03
Attitude toward Religious Influence on Society	–.15	–.02	–.20
Liberalism Self-Identification	–.22	–.03	–.21

[a] $R = .26; a = .10; N = 26.$

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[8] Copies of the attitude and role orientation (see below) scales are available from the author. Principal components factor analysis (with varimax rotation where more than one factor emerged) was used to generate factor scores for each of the scales used in this analysis. The first factor is the single best summary measure of the variance in the original items. Throughout the analysis the first factor is used to produce factor scores, which are used as the measure of the concept. The process is simply one of creating summated indicies, except that the weight of each of the variables is not 1 (it is the factor score coefficient). The fact that in several instances more than a single factor emerged from a group of items which were designed to measure a single concept should be of only limited concern. Many of the scales form Guttman scales and Guttman scales, when factor analyzed, are always "two dimensional." A second influence which may account for a higher order of dimensionality in the factor results is related to the small number of judges on which the factor analysis is based. With a small number of observations, the error component is likely to be rather large and hence to be strong enough to produce its own factor. Consequently, it is believed that many of these higher order factors represent "noise" in the data and are of little substantive concern.

The judges were also asked to rank several national problems in terms of their seriousness. This question generated the third criminal justice belief variable—the judges' evaluation of the seriousness of crime as a social problem. The variable is simply measured by the rank which the judge assigned to the problem of crime. It is hypothesized that judges who see crime as more serious will be more severe in sentencing.

The judges' positions on several criminal justice issues constitute the fourth belief about criminal justice. The issues are: (1) allowing six-member juries in criminal cases, (2) allowing non-unanimous verdicts in criminal cases, (3) reactions to the "Miranda" rules, (4) providing free legal counsel to indigents in *all* criminal cases, (5) the use of plea bargaining, (6) gun control, and (7) the use of the death penalty. The first four items were measured by a "favor-oppose" response set, while the last three asked the respondents to indicate which of several statements came closest to the way they felt about the issue. The measure used is a summated scale of the responses, with a maximum value of 14. Favoring smaller juries, non-unanimous verdicts, plea bargaining, and the death penalty, and opposing the Miranda rules, free legal counsel and gun control are viewed as antidefendant positions. I hypothesize that judges favoring antidefendant positions will be more severe in sentencing.

The fifth measure concerns the legalization of private behavior. Each judge was asked to evaluate (1) decriminalizing all sex acts of mutual consent, (2) legalizing gambling, and (3) the general trend toward decriminalization. Factor scores were produced for this variable. Attitudes in support of the legalization of private behavior are hypothesized to result in lenient sentencing behavior.

Table 3 reports the results of the bivariate correlations and multivariate regression for the five attitudes and sentencing behavior. The data demonstrate that judges' attitudes toward crime and criminals have little impact on their sentencing behavior, just as their political liberalism had little influence. Even though these attitudes are more closely related to the day-to-day operation of the criminal justice system, they apparently have little influence on the judgments produced by the system. Sentencing behavior varies independently of these five attitudes of the sentencer.

Further investigation of these data merely confirms the lack of relationship between attitudes and behavior. For instance, I inspected the scattergrams to determine whether curvilinear effects had depressed the linear correlation coefficients. They did not. Even a multiple regression analysis using all nine attitudes supported the finding of little relationship. Together, the nine attitudes account for only 14 percent of the variance in sentencing behavior. The regression indicated that the two best predictors of behavior are the judges' attitudes on the legalization of private behavior (beta = .29) and their attitudes on the criminal justice issues (beta = .27). The other attitudes make rather minor contributions to explaining sentencing variance. Thus, these findings cast significant doubt on the traditional approach to predicting judicial behavior from attitudes: the model Behavior = f(Attitudes) is inadequate.[9]

Table 3. Criminal Justice Attitudes and Sentencing Behavior[a]

Criminal Justice Subdimensions	Pearson r	b	Beta
Attitude toward Causes of Criminal Behavior	.14	.03	.17
Goals in Sentencing	.03	–.01	–.05
Perception of Seriousness of Crime Problem	–.16	.02	.15
Attitude toward Criminal Justice Issues	.10	.03	.18
Attitude toward Legalization of Private Behavior	.03	.02	.09

[a] $R = .26; a = -.33; N = 25$.

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[9] It can be argued that the attitude-behavior model is still adequate, the problem being that the measures of attitudes do not include the attitudes which are "really" important. Political liberalism and criminal justice beliefs may be too diffuse to have much impact on sentencing. So that I could assess this argument, I included a question in the questionnaire which directly measured the propensity of the judge to give severe sentences in specific types of crimes. The responses were correlated with the actual sentences the judges gave for each type of crimes. While the number of cases on which the actual sentence scores are based is frequently quite small, the data clearly indicate that the judges' attitudes toward the severity of these crimes do not influence their behavior. (Further details on the analysis can be obtained from the author.)

Role Orientation as an Intervening Variable

The correlations between the judges' attitudes and the severity of their sentencing behavior are less than spectacular. This is not entirely surprising: nonjudicial experimental research on the relationship between attitudes and behavior have reported similarly unsubstantial correlations. However, these same experimental studies have also made some significant advances in dissecting the nature of the low correlations. Several scholars have suggested that the relationship between attitudes and behavior is probably mediated by situational intervening variables. That is, the relationship between attitudes and behavior is *modified by the situational context of the* behavior. Reference group variables, the subject's definition of the situation, social participation, need for approval, social constraints and social distance, the consequences of behavior, and a wide variety of other variables have been identified as significant contextual variables.[10]

The most relevant formulation of the role of intervening variables, however, is found in Campbell's notion of situational thresholds (Campbell, 1963). Similar attitudes may result in different overt behaviors due to situational pressures which inhibit the expression of behavior consistent with the attitude. The impetus for a response must be strong enough to overcome situational forces inhibiting the response. Consequently, we can predict that an individual would give a discriminatory response to a questionnaire item (low threshold) but fail to discriminate in face-to-face situations (high threshold). Thus, there is little question that the actor's assessment of the context of behavior affects the degree of relationship between attitudes and behavior.

Situational variables can be summarized most usefully by the concept "role expectations." Individuals who interact with role occupants have conceptions of what constitutes "proper" role behavior for the role occupant. These are norms of behavior which constrain the activities of the role occupant. Situational constraints and "contingent consistency" are concepts highly related to role expectations. While expectations affect almost all social behavior, they are especially salient for actors in positions subject to relatively unambiguous, institutionalized role expectations. A role orientation is a psychological construct which is the combination of the occupant's perception of the role expectations of significant others and his or her own norms and expectations of proper behavior for a judge.[11]

The concept of role orientation plays a central function in this analysis. Role orientation is essentially a summary variable which defines for the role occupant the range of appropriate behavioral alternatives in any given situation. As such, role orientations are very similar to many of the situational intervening variables identified above and are almost identical to Rokeach's (1968) notion "attitude toward situation" (A_S). Role orientations are also similar to Campbell's situational threshold. In order for an attitude to find expression in behavior, the behavior consistent with the attitude must lie within the range of acceptable behaviors, i.e., be defined as situationally appropriate.

While many judicial researchers have investigated judicial role orientations, none has done so from this perspective. Most studies have been content to develop empirically based typologies. For instance, basing their conclusions on interviews with 26 Supreme Court justices from Louisiana, New Jersey, Pennsylvania, and Massachusetts, Glick and Vines have identified four types of purposive role orientations: (1) the ritualist, (2) the adjudicator, (3) the policy maker, and (4) the administrator. Decision-making role orientations include: (1) the law interpreter, (2) the lawmaker, and (3) the pragmatist (Vines, 1969; Glick, 1971; Glick and Vines, 1969). Ungs and Baas (1972) have also developed a similar typology based on the responses of Ohio trial and appellate judges. Howard (1974a) also has developed similar categories. Flango, Wenner and Wenner (1975) have refined some of these typologies and extended them to non-American judges. Nevertheless, few researchers have attempted to relate role orientations to judicial behavior. To do this it is useful to think of a role orientation as judges' beliefs about what variables can properly be allowed to influence their behavior. Becker, among others, has suggested that the most important of these variables is precedent. He offered some empirical support that judges' orientations toward precedent can be used to predict their behavior (Becker, 1966). However, Flango and Schubert (1969) presented contradictory evidence on this point. Grossman (1962) suggested a similar hypothesis with respect to Justice Frankfurter's votes in civil liberties cases, while Schubert (1963) has demonstrated that precedents affected some U.S. Supreme Court justices in several military law cases. Howard (1974b) suggests that the influence may be issue-specific. While these scholars seem to agree that the judge's orientation toward precedents is an important intervening variable, none has been successful in developing an empirical model integrating attitudes, role orientations, and behavior (Gibson, 1977a).

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[10] See for examples of this type of research the articles reprinted in Deutscher (1973) and Thomas (1971).

[11] This is not to assert that role orientations perfectly mirror either role expectations or the role occupant's own role expectations. Role orientations reflect what individuals think they ought to do, tempered by what they think others think they should do. It is also possible that non-normative expectations, such as those reflected in power relationships, influence the acting-out of role orientations. How these factors can be identified and measured is not apparent.

At least a portion of the difficulty in relating role orientations to decision behavior stems from the inadequacy of the present conceptualization of role concepts. Judges' role orientations are their beliefs about the kind of behavior proper for a judge. In the case of decisional role orientations, the beliefs concern what constitutes proper decision-making behavior. "Proper" does not, however, refer as much to the kind of policy which is made as it does to the kind of stimuli which influence policy making. The basic function of decision-making role orientations is to specify what variables can legitimately be allowed to influence decision making, and in the case of conflict, what priorities to assign to different decisional criteria. Some judges may believe it proper to be influenced by a particular stimulus while other judges may regard the stimulus as improper.

There are good reasons for conceptualizing role orientations in this way. A central expectation of judicial and legal traditions concerns the criteria of decision making employed by judges. For instance, "equality before the law" is not an empirical statement; it does not assert that individual litigants are in fact equal. Rather the phrase is an exhortation to ignore the variables (stimuli), such as power, on which litigants are unequal and to render decisions only on variables which provide for equality. As an additional illustration, it is generally regarded as illegitimate to discriminate on the basis of race in sentencing. This means that it is illegitimate to allow the race of the defendant to influence the decision. Race is not viewed as a proper decisional criterion. Similarly, even such concepts as the presumption of innocence in criminal cases are merely expectations that court officials will not allow empirical stimuli relating to the factual guilt of the defendant to influence their pretrial decisions (e.g., bail). The presumption of innocence is a norm which defines some criteria of decision making as proper and others as improper (Packer, 1968).

Thus, role orientations specify the criteria upon which decisions are made. A generalized role orientation might concern the legitimacy of allowing criteria which have no strictly legal base to influence decision making. A "broad" role orientation restrains the use of other criteria very little. The "narrow" orientation is more restrictive in the use of "nonlegal" criteria. What is important to note, however, is that the role orientation does not indicate whether judges' policy decisions will be liberal or conservative, but instead indicates the criteria upon which their decisions will be based. For a decision to be liberal, the theory predicts that the criterion upon which the judge bases a decision must itself predict a liberal decision. Two factors must therefore be known in order to predict decisions: (1) what criteria the judge considers to be legitimate determinants of decisions, and (2) whether the criteria themselves predict liberal or conservative decisions.

There is no logically *necessary* basis for expecting judges with "broad" role orientations to make "liberal" decisions unless the decisional criteria employed by "broad" judges are liberal. One might imagine, for instance, that judges with "broad" role orientations would grant greater legitimacy to allowing their own values and sense of justice to influence their decisions than would "narrow" judges. If the values are liberal (as in the case of William O. Douglas, for instance), then the decisions will be liberal. If the values are conservative, however, then the decisions will be conservative (as illustrated by William Rehnquist). Obviously, this suggests that "activism" can just as well be conservative "activism" (e.g., the Hughes Court) as liberal activism. Similarly, a "narrow" orientation does not necessarily lead to conservative decisions. If "narrow" judges tend to grant greater legitimacy to criteria with a strictly legal base (e.g., precedents, statutes, and "strict construction" of constitutions), and if those criteria were liberal, then a "narrow" judge would make liberal decisions. For instance, the Burger Court, if it were acting "narrowly," with strong regard for Warren Court precedents, would make liberal decisions. Further, a "strict construction" of the First Amendment prohibition on laws restricting freedom of speech would result in very liberal decisions (at least up to the question of "symbolic speech"). Thus, role orientations should not directly predict decisions but, in combination with the decisional criteria a relationship should exist.

Consequently, role orientations, attitudes and behavior can be integrated into one model; attitudes are related to behavior only insofar as the judge's role orientation allows it. Judges viewing attitudes as having a legitimate role in decision making allow their personal values to influence their decision making.[12] This formulation is amenable to direct empirical evaluation.

Regression residuals can provide a good measure of the extent to which individual judges' attitudes predict their behavior. Using the nine attitudinal measures as independent variables and the sentence severity score as the dependent variable, I calculated these residuals. A small (absolute value) residual indicates that the attitudes are strong predictors of sentence severity; a large residual indicates that attitudes are weak predictors of severity. Thus, the hypothesis is that the judge's role orientation is related to the degree to which behavior is influenced by attitudes (the residuals).[13]

The measure of role orientations, like the attitudinal measures, is a standardized factor score resulting from factor analyzing six Likert items. These items all measure the judge's conception of proper judicial decision making, and specifically concern the legitimacy of relying on their own attitudes and values, in contrast to precedent, statutes, etc., in decision making. Low scores indicate that the judge accepts attitudes as a legitimate decisional criterion and sees a somewhat limited role for precedents. This will serve as the measure of role orientations.[14]

Figure 1 shows the plot of the regression residuals and the role orientation factor scores. The Pearson correlation is .51. The results are just as hypothesized: role orientations are acting as an intervening variable mediating the attitude-behavior relationship. Attitudes are good predictors of behavior only for those judges with "broad" role orientations. Role orientations do *not* predict the severity (policy content) of the sentences (r = .29); they do, however, predict how much influence certain criteria will have on behavior.

These data clearly indicate that role orientations intervene between the judges' attitudes and their behavior. Role orientations do significantly predict the criteria, but not the policy content of decision-making behavior (at least in terms of sentencing). The question which re-

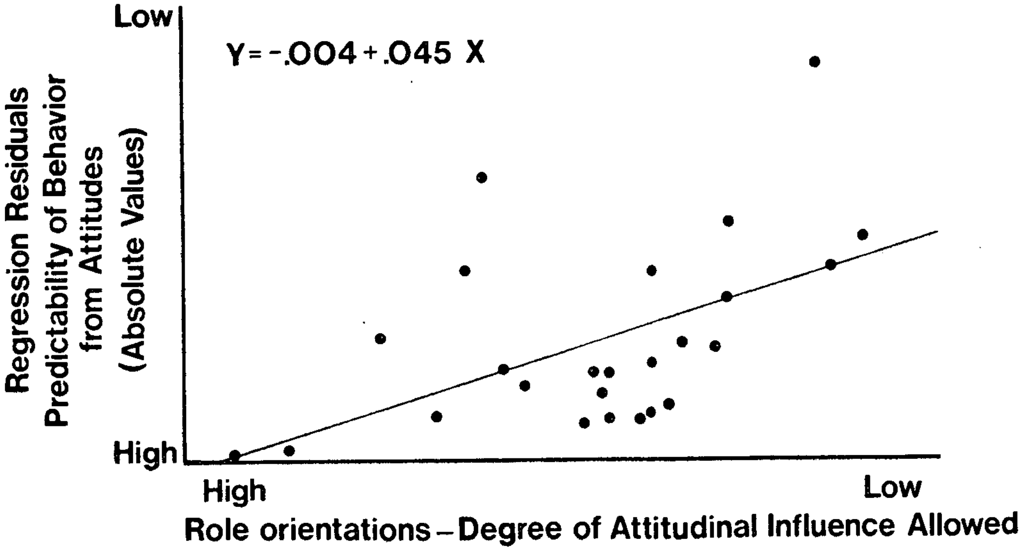

Figure 1.

Scattergram of Role Orientations and Regression Residuals mains is: what does predict policy content? The answer to this question is not entirely straightforward. It is possible, however, to assess the degree to which attitudes influence sentencing behavior for those judges who view such influence as legitimate. The approach is to first dichotomize the role-orientation factor score at the mean (or median, the groups are the same), resulting in two groups of judges. I then calculated the correlations between the nine attitudes and the sentence severity scores for each group of judges *separately*. Judges with role-orientation scores below the mean are hypothesized to have a greater correlation between their sentencing behavior and their attitudes about criminal justice and liberalism than judges who are above the mean score for the role-orientation factor. The results are shown in Table 4.

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[12] This, of course, is not to deny that assuming a role orientation is very much a function of the judge's own values.

[13] The residuals were calculated in the normal fashion: residual = $(y_i - \hat{y}_i)$. $\hat{y}$ is calculated from the coefficients listed in Tables 2 and 3. A residual is simply the deviation of an individual from what is predicted on the basis of the linear regression. The measure of attitudinal influence is the absolute value of the residual.

[14] While there are obviously other dimensions of judicial role orientations, the research is specifically designed to investigate the impact of role orientations on decisional behavior so the measure focuses specifically on the judges' conceptions of legitimate decisional criteria. The items and the results of the factor analysis are available upon request from the author.

The data clearly show that judges who view attitudes as legitimate criteria of decision making are strongly influenced by their attitudes: the adjusted coefficient of determination (adjusted R^2)[15] of the attitudes and sentencing behavior is .44. Judges who view attitude influence as proper are especially influenced by their beliefs about class relations in the U.S. and their beliefs about the causes of criminal behavior. Activists who see divergent social classes and who believe crime is environmentally determined are very much more sympathetic to defendants. Thus, for at least a significant minority of judges, attitudes are a key determinant of behavior.

The causes of the sentences given by the remaining judges are not as easy to determine. Attitudes aid comparatively little in explaining their behavior (adjusted R^2 = –.46). Previous research has suggested that this type of judge relies more heavily on the recommendation of the district attorney (Gibson, 1977a). A similar analysis can be conducted on these judges. The judges were asked to respond to the following question: "How influential do you think the following factors should be in sentencing defendants found guilty in criminal court?" The response set ranged from extremely influential to uninfluential. The correlations between each of the factors and the judges' role orientations are shown in Table 5.

The data are still somewhat inconclusive on the question of what judges with "narrow" role orientations base their decisions on. While judges with "broad" orientations, as expected, rely more heavily on such extralegal variables as the attitude of the defendant, the severity of the crime, their own values and public opinion, they are also more influenced by the prior record of the defendant. Indeed, there is a tendency for "broad" judges to rely on almost all criteria more heavily. Conversely, "narrow" judges rely slightly more heavily on the recommendation of the district attorney.[16] Conse-

Table 4. The Intervening Effect of Role Orientations on the Attitude-Behavior Relationship

Attitude	Below Mean Role Score (Activists) *r*	*N*	Above Mean Role Score (Nonactivists) *r*	*N*
Criminal Justice				
Attitude toward Causes of Criminal Behavior	.58	12	–.10	14
Goals in Sentencing	.17	12	–.08	14
Perception of Seriousness of Crime Problem	.37	12	.05	13
Attitude toward Criminal Justice Issues	.10	12	.02	13
Attitude toward Legalization of Private Behavior	.11	12	.13	13
Political Liberalism				
Attitude toward Government Economic Intervention	–.01	12	.04	14
Class-Based Beliefs	–.66	12	.23	14
Attitude toward Religious Influence on Society	.02	12	–.29	14
Liberalism Self-Identification	–.56	12	–.03	14

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[15] Adjusted R^2 can aid in the comparison of the coefficients for the two groups of judges since the Ns are different and quite small. Adjusted R^2 corrects for the degrees of freedom so that R^2 for different groups can be compared (Johnston, 1972, pp. 129–30). For the 12 judges who viewed attitudes as legitimate criteria unadjusted R^2 = .92. For the remaining 13 judges the coefficient is .63.

[16] This assertion is empirically supported by correlating the role orientation measure with a measure of the number of criteria which the judge rated as "extremely influential." The correlation is –.52, indicating that "broad" judges are more likely to rely heavily on more criteria than "narrow" judges.

quently, while the data do provide some further clues to the behavior of the "broad" judges, they are still relatively unenlightening in regard to the "narrow" judges.[17]

To summarize: this analysis began with fairly small correlations between judges' attitudes and their sentencing behavior. The low correlations are apparently due to the inadequacy of a simple linear model of decision making, and they strongly suggest the necessity of considering the intervening effect of the role orientations of the judges. The policy content of the sentencing behavior of "broad" judges is now fairly predictable; the behavior of the remaining judges is not so well understood.

Table 5. Role Orientations and the Determinants of Sentencing Behavior

Determinant[a]	Correlation with Role Orientation[b]
The recommendation of the district attorney	–.09
The attitude of the defendant	.39
The prior record of the defendant	.42
Public opinion	.20
The severity of the crime	.27
Whether the defendant pleaded guilty or was found guilty by a jury	.03
The recommendation of the pre-sentence report	.13
The defendant's sex	–.07
Your own personal philosophy and values	.23

[a] The question used was: "How influential do you think the following factors should be in sentencing defendants found guilty in criminal court?"

[b] N = 26.

An Interactive Model of Role Orientations, Attitudes, and Decisions

The objective of this research has been to develop a model and theory of the interrelationships among judges' attitudes, role orientations, and behavior. While the utility of the model was demonstrated in the previous section it is possible to describe the effect of attitudes and role orientations in a somewhat more simplified fashion. A regression equation of the form

$$Y = b(ZX_1) + b(ZX_2) + \ldots . + b(Z_nX_n)$$

where Y = the sentence severity scores, X_1 = the attitudes of the judge, and Z = the role orientation of the judge, statistically describes the theory of the interaction of attitudes and role orientations. Using this equation, we may assess the utility of the model for explaining the behavior of the Iowa trial court judges. While the small N limitation still applies, it is instructive to examine the data with this equation.

Each of the nine attitudinal variables identified in Tables 2 and 3 were first multiplied by the role-orientation factor score. I then converted them to standard scores, and ran a multiple regression analysis with the mean standardized sentence severity scores as the dependent variable. The results strongly support the use of the equation. The squared multiple correlation coefficient (R^2) for this interactive equation was .64, which means that almost two-thirds of the variance in the sentencing behavior of the judges can be accounted for by the interaction of attitudes and role orientations! This compares to a multiple R^2 of only .14 for the correlation of the nine (noninteractive) independent variables and the sentence severity scores. A difference of this magnitude cannot be attributable to the small number of judges. Thus these data are extraordinarily supportive of the theory: attitudes alone account for 14 percent of the variance in sentencing behavior; role orientations alone account for 8 percent of the sentencing variance; while the interaction of attitudes and role orientations can explain 64 percent of the variance.

Conclusions

This research was designed to develop a model of the interrelationships among judges' role orientations, attitudes, and sentencing behavior. I generated measures of the role orientations and attitudes of the judges from personal interviews and constructed a measure of sentencing behavior from court records on nearly 3,000 criminal convictions. Not surprisingly, the bivariate correlations between the attitudes of the sentencer and the actual sentences were quite low: the findings are consistent with experimental attitude-behavior studies as well as some previous judicial research. The reason for the lack of significant correlations can apparently be attributed to the effect of an intervening variable, role orientation. A role orientation specifies for the role occupant the range of legitimate criteria of decision making. Thus, some judges believe that it is not proper for them, as judges, to allow their own values to influence their decisions. The data demonstrated that the role orientations of judges do indeed block the relationship between attitudes and behavior, a finding also consistent with the experimental literature. The correlation between the role orientation and the degree of influence of attitudes on sentencing was .5. The data show that for some judges attitudes are extremely influential in their sentencing decisions; for others, attitudes are inconsequential. Role orientations do not directly predict the policy content of behavior; rather, they predict the criteria of decision making. This may also explain why previous studies of role orientations have not been successful in predicting judges' behavior. Finally, an interactive regression model was presented which was capable of explaining two-thirds of the variance in sentencing.

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[17] As this research demonstrates, the interactions among attitudes, role orientations, and behavior are complex. When consideration of case and defendant characteristics is attempted theoretical and methodological problems increase exponentially. For instance, consider how the sex of the defendant might affect decisions: first, some judges may not even recognize sex as a decisional stimulus: sex is simply not salient to them. Among those to whom sex is salient, differences exist in perceptions of the legitimacy of relying on this criterion in making decisions. If sex is viewed as legitimate and if the judge has sexist attitudes then a difference will exist in the sentences given males and females. If the judge's attitude is nonsexist, then no difference will be apparent. Thus, nondiscriminatory behavior will be exhibited by judges who (1) do not perceive sex as salient, and/or (2) do not believe sex is a legitimate decisional cue, and/or (3) have nonsexist attitudes. Obviously, a complex simulation is the only means by which the process can be modeled. Research in this direction is currently in progress.

The generalizability of this model of decision making is indeed limited by the small number of judges analyzed, the lack of comparative data, and the lack of data on other important decisions criminal court judges make (e.g., decisions on bail, pretrial motions, and, in some jurisdictions but not in Iowa, the guilt or innocence of defendants). The model which I have employed is not so limited, however. Any instance of decision making in which some decisional criteria are considered legitimate and others are not considered legitimate is susceptible to analysis by this model. Whenever decision makers are subject to role expectations concerning the criteria of decision making the model may be useful. By conceptualizing the process as essentially two-dimensional (*how* decisions are made and *what* decisions are made) and by examining the relationship between the two dimensions, the model is capable of bringing together a variety of previously unrelated theories of decision making (e.g., attitude theory, role theory, cue theory, information integration theory, etc.) with the ultimate result of a more generalized theory and a greater ability to explain and predict complex decision-making processes.

Nor is the model limited to the effect of judges' attitudes on their behavior. The theory can be applied to virtually any criteria of decision making, and any type of decision, as long as measures of the criteria, and measures of behavior are included in the research. For instance, this model has been profitably extended to the decision criteria of public opinion (Gibson, 1976), and the sentence recommendations of district attorneys (Gibson, 1977b). The model might even be applied to fact pattern analyses. For instance, recent analysis by Ulmer (1974) indicates that members of the U.S. Supreme Court differ in the degree to which they are influenced by "non-legal" case characteristics. This may be a function of the intervening effect of the role orientations of the justices; that is, they may differ in their beliefs about the legitimacy of allowing non-legal criteria to influence their decisions. In principle, at least, there seems to be little obstacle to applying the model to other decisional criteria. Given a sufficient number of judges the equation

$$Y = b(Z_1X_1) + b(Z_2X_2) + \ldots + b(Z_nX_n)$$

where Y = the decision, X = any decisional criteria, and Z = whether the criterion is considered legitimate by the judge, has great potential for explaining the behavior of judges.

Finally, the model may also be useful for understanding decision making in nonjudicial institutions. Indeed, role orientations may provide an appropriate vehicle for analyzing inter-institutional differences in decision making in a more systematic fashion. Differences among institutions in the predictability of behavior from various decisional criteria may be a function of differences in the contextual constraints imposed on decisions through role expectations and role orientations. If a common pool of criteria could be identified, a truly general model could be developed to explain judicial and legislative decisions, for instance. Judges and legislators may differ only in their perceptions of the legitimacy of various decision-making criteria. When institutional differences are treated as role-related variables, this single general equation allows the behavior of different decision makers to be parsimoniously analyzed.

This research has also shown how much utility there is in combining several variables in decision-making models. Considered separately, neither attitudes nor role orientations adequately explain sentencing behavior; it is only when they are placed in a multivariate model that a significant amount of variance is explained. Certainly, the addition of measures of more and different concepts to the decision process model will increase the ability to predict decisions (that is, beyond statistical artifacts). Judicial decision making is simply too complex to be adequately characterized by bivariate theories or models. Future efforts to understand judicial behavior should be sensitive to this fact.

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Bickel's Constitution: The Problem of Moderate Liberalism

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Alexander Bickel's three most comprehensive books explore a common constitutional-political theme, the manner in which sound political judgment should guide judges and scholars who authoritatively interpret the United States Constitution. Yet the works differ, and the differences illuminate a dual development of Bickel's understanding: a growing fear of the contemporary obstacles to politic constitutional judgment, and a growing thoughtfulness in coming to grips with these obstacles. The Least Dangerous Branch *had invented politic techniques for applying the judiciary's principles.* The Supreme Court and the Idea of Progress *cautioned against judicial application, by novel techniques, of an impolitic egalitarian faith.* The Morality of Consent, *upon which this paper concentrates, elaborates Bickel's turn from the techniques of judicial power to the wise direction of judicial power. The paper considers the direction that Bickel proposes.*

Alexander M. Bickel (1925–1974) was a scholar of constitutional law and a student of our politics, if the two can be properly separated. He thought they could not. He wrote scholarly articles for law reviews, scores of essays on law and politics for journals such as the *New Republic* and *Commentary*, and nine books. Two, *The Caseload of the Supreme Court* and *The Unpublished Opinions of Brandeis*, could be labeled chiefly legal in subject. Two, versions of a study of electoral college, convention, and party system, might with equal hesitation be called political. The other five are addressed even by their titles to the relation of law and politics. This is especially true of the three most comprehensive works, to which this essay is limited: *The Least Dangerous Branch: The Supreme Court at the Bar of Politics* (1962), *The Supreme Court and the Idea of Progress* (1970), and *The Morality of Consent* (1975). These indeed share a common constitutional-political theme. Each explores the way in which sound political judgment should guide those—judges especially, but also legal scholars—who authoritatively interpret our Constitution. These works differ, however, and become increasingly controversial. Their important differences exhibit a dual development of Bickel's understanding: a growing fear of defects in the constitutional judgment of characteristically modern judges and scholars, and a growing thoughtfulness in considering remedies for these problems, problems glossed over in his earlier formulations.

Bickel's final book, *The Morality of Consent,* was published posthumously in 1975 and is his culminating work, albeit not a fully satisfying one. It portrays a contemporary crisis of practical judgment that originates, however, in a crisis of theory. Moderate judging and thus moderate institutions are said to be endangered by two impolitic extremes of liberal or democratic theory. "Our problem has been, and is most acutely now, the tyrannical tendency of ideas and the suicidal emptiness of a politics without ideas" (1975, p. 12). Bickel seems to think our institutions endangered by a peculiar assault and by a peculiar decay, both engendered by peculiar "ideas." We are victimized simultaneously by a committed "moralism" that tends to a "dictatorship of the self-righteous," and by a permissive and even nihilistic moral relativism that eats away at the social fabric (1975, p. 142). This last work of Bickel seeks to restore the views and the judiciousness needed, as he thought, by our sort of representative government and free society. These he calls a "morality of consent," for complicated reasons to be discussed. Bickel has been somewhere described by George Will as "the keenest public philosopher of our time." Will thus recalls Walter Lippmann's term for the comprehensive liberal persuasion that Lippmann thought, even 20 years ago, in need of restoration (Lippmann, 1955, pp. 113–40).

Principle Without Prudence: The Deeper Problem

The first and still most praised of Bickel's major works was *The Least Dangerous Branch* (1962). It explored the disposition proper to the modern Supreme Court and concluded that the task of judicial prudence was the "accommodation," by certain arts of compromise, of the tension between enduring "principles" pronounced by "authoritarian judicialism" and "the practice of democracy" (1962, pp. 28, 40, 244). Note that Bickel begins by focusing upon the prudence of judges and not the meaning of constitutional provisions. This fundamental orientation seems to follow partly from Bickel's debt to Oliver Wendell Holmes' critique of the old or Marshallian constitutionalism, and partly from moral and political doubts about the old orientation by property rights and laissez-faire. Holmes had replaced the old understanding of a fixed Constitution, fixed rights, and fixed "axioms of government" (such as the doctrine of separated powers) with an historical account, an account of law as pronounced by judges and yet as attuned to the evolution of society. Judges thus gained a sort of preeminence over their old constitutional bonds, and were set the task, no mean task, of discerning and following the "path of the law" through ever-changing "contemporary" social conditions and ideas (Holmes, 1952, pp. 175–86; cf. Faulkner, 1968, pp. 235–48). As Edward Purcell has shown in a valuable article, Bickel agreed with Holmesian realism in denying the "fictions" of traditional jurisprudence, in denying the old atavistic "simplicities" that judges "found" law and that their choices might be mechanically derived from the Constitution or governed by "logic" (1962, pp. 74, 69; Purcell, 1976, p. 520). Bickel's works share the fateful agreement of "all modern writers" (including his friend and teacher Felix Frankfurter) in thinking our judicial problems radically different from those known to the founders, and our judicial "value choices" thus new matters of policy (1962, p. 39). Yet there was more to Bickel's basic posture than Holmes' ostensibly methodological and neutral historicism. Bickel turned from the purposes and literal meaning of the Constitution in part because he was repelled by what he thought he would find. To inquire into the motive or purpose perhaps implied by the Constitution's grant of powers, say, over commerce, might inadmissibly restrict Congress in its modern regulation and redistribution of the economy and its fruits (1962, pp. 162, 184, 208 ff.). Similarly, to follow the Fourteenth Amendment literally might require the Court to allow perpetuation of segregation in schools, an "inadmissibly static view of society" (1962, pp. 249 and 256, 44–45). If Bickel is a "realist" he is a "progressive realist," as he later calls Frankfurter (1970, pp. 18–20). Indeed, various parts of *The Least Dangerous Branch* are expressly given over to a defense of the Court's course in *Brown* v. *Board of Education* (347 U.S. 483 [1954], 349 U.S. 294 [1955]), of its mixture of authoritative and equalizing principle with a certain practical toleration of all deliberate speed.

The judiciary's new preeminence above the Constitution ought not entitle it, however, to follow mere whim or impulse. Bickel attacked not only the constitutional literalists, be they Chief Justice Marshall or Justice Black, but also those "legal realists" derivative from Holmes who traced legal decision to mere "personal predilection." If this were true, the newly realized authority of judges would be "intolerable." Besides, it is not true. "Reason" and "the maturing of collective thought" visibly inform judicial determinations. Although the life of the law is not logic, the life of the law is nevertheless "reason" (1962, pp. 237, 80, 84, 197). Bickel then follows Herbert Wechsler in holding that courts should abide by "principle." Moved by Harry Jaffa's account of Lincoln's judiciousness in providing for the substantive principles of the Declaration of Independence, however, Bickel suggests a need for principled "goals"; he deprecates Wechsler's own emphasis upon "generality" and "neutrality" (1962, pp. 65–72). It is "decidedly the Court's function to proclaim principled goals" (1962, p. 141).

As to which goals are principled, and which principled goals are to be proclaimed, Bickel does not imitate Lincoln's clarity. He seems distressingly vague. He occasionally even substitutes the loose contemporary term "values" for "goals": to a Supreme Court so understood accrues the authority of "pronouncer and guardian" of "enduring values" (1962, p. 24). Still, in *The Least Dangerous Branch* and later works, Bickel tends to restrict the Court to "fundamental" values or to "principle," however these may be defined. The question of definition we touch later.

In any event, Bickel's effort to guide the Court dwells less on clarifying its moral principles and more on calling for "restraint" in the face of another sort of principle. The Court must show the restraint needed in a society "dedicated both to the morality of government by consent and to moral self-government" (1962, p. 199). Judicial "domination" would be tyrannical in a democracy like ours. Besides, principles govern only as obeyed, and in a democracy only by virtue of a large measure of voluntary consent. *The Least Dangerous Branch* then prescribes a judicial course intended to provide for both judicial efficacy and political democracy. One needs to find "means . . . for providing a wide-ranging and effective rule of principle, while at the same time eschewing full dominion and affording the necessary leeway to expedient accommodation" (1962, p. 200).

Bickel set out with grace and subtlety to clarify these complicated "accommodations." He discusses devices for avoiding decision of a case (doctrines requiring standing to sue, ripeness of a case for decision, and a judicial rather than a political question) and devices for avoiding constitutional questions (doctrines as to vagueness of a statute and as to improper delegation of executive or legislative power). In general, he celebrates "passive virtues" and even makes the apparently paradoxical claim that his "legalisms" of inaction are "the most important thing" (1962, pp. 112–13). Still, what Bickel does is neither as strange nor as passive as might appear. He understands his counsel of passivity to be fundamentally a tactic of activity; judicial efficacy on behalf of "fundamental values" takes priority over judicial restraint. Bickel has in fact transformed old legal doctrines, thought to be limits inherent in judicial power, into new tactics of judicial power. He replaces axioms of traditional jurisprudence with techniques of mediation. These turn out to be techniques of hidden government, ways to jolly a political process, all too governed by "interests" and "expediency," toward the judiciary's principles. A contemporary sociologist has gone so far as to speak of the rise of "an imperial judiciary" (Glazer, 1975). Bickel seems to point toward some kind of new judicial authority, with powers retaining only the odor of the familiar old restraints. Judges may withdraw from unfavorable cases; in deciding cases they may at least refrain from giving "legitimacy" to the views of the "majority forces of order, which will speak for themselves readily enough"; they can refrain from a confrontation with gradually enlightening opinion (1962, pp. 143, 153). More positively, judges can send up "trial balloons" and engage in "colloquy" with prevailing opinion (especially that of lawyers and law teachers), thus to render their "answers familiar, if not obvious" (1962, pp. 240, 156, 176). Indeed, they are "teachers to the citizenry," it seems, sent to mold the people's view of durable principles of government (1962, pp. 69, 235, 240). The indefiniteness of Bickel's understanding of "self-government" thus gives way to his arming of a principled court, however indefinite his understanding of its principles. The least dangerous branch has been shown how quietly to become newly powerful, if not most powerful.

Eight years later, *The Supreme Court and the Idea of Progress* shocked much of the legal establishment. Bickel had revolted from his self-appointed role as moderate tactician for the "wide-ranging and effective rule" of the Supreme Court's principles. "I have come to doubt in many instances the Court's capacity to develop 'durable principles,'" he announced, "and to doubt, therefore, that judicial supremacy can work and is tolerable in broad areas of social policy" (1970, p. 99). Courts now seemed "a most unsuitable instrument for the formation of policy," not least because of their remoteness from the interests and deeds and concerns of those governed. They are *too* "principle-prone and principle-bound" (1970, p. 175).

Although *The Least Dangerous Branch* had favored much the rule of principle, we have noted, it had not clarified much *which* principles, or their merit, or their moral or economic or political results. Despite being the most elegantly detailed of Bickel's books, its core is somewhat evasive and paradoxical, as Purcell noted (1976, pp. 542–43). Principles were to be moral, to express "ultimate values," and decidedly not to be drawn from the "marketplace of political interests" (1962, p. 79). These touches of a Kantian moralism preoccupied with universal principles were rendered even more indefinite by their source: principles are distinguished not by clear content but by their origin in our particular "history." They are to be "evolved" by the judiciary out of the "evolving morality of our tradition" (1962, p. 236). Little wonder that only "evocations" and not "definitions" could be given of any child of such a mismatch: mere generalities, said to be enduring, are mated with a mere historical process, said to be evolving (1962, p. 199). A reader glimpses some indication that an "open" society with less censorship, more "integration" of groups whose dignity has been hitherto denigrated, and abolition of inhumane rites like capital punishment, makes up the principled goal Bickel had in mind (1962, pp. 57, 141, 232–33, 236–38). Bickel could tolerate such obscurity as to the use of his armory of techniques only because of his post-Holmesian historical faith; "society" is believed to be moving forward, "evolving."

This complacent supposition, together with the abstract equalizing it shields from question, is *the* target of *The Supreme Court and the Idea of Progress.* The Warren Court in particular had entertained a "faith in progress" that allowed it to "wreak itself upon society" without due consideration and too often, Bickel had come to think, with harmful effects often contrary even to its own intention. Thinking that "history would validate their deeds," the justices too often refrained from thinking them through. This new generation of "Progressive realists" had supposed that judges might be self-conscious "statesmen" conducting the country to "man-made progress" by real knowledge of the "industrial and political conditions now before us." They proved as susceptible as their old predecessors, however, to the promulgation of subjective dogma in the name of progress (1970, pp. 19–20, 39). Still, Bickel goes beyond the Hart-Wechsler charge of "incoherent" and "unprincipled" decisions, as Judge J. Skelly Wright soon pointed out in a pungent rebuttal (Wright, 1971, p. 722). Bickel challenged "the supporters of the Warren Court on their own substantive result-oriented ground." Beyond a faith still basically shared by commentators as diverse as McCloskey, Cox, Pritchett, and Kurland, Bickel alone deeply questioned the Warren Court's substantive pursuit of a "theoretic" equality. *The Supreme Court and the Idea of Progress* was more uneven, more searching, and certainly more controversial than *The Least Dangerous Branch,* as its author sought to think his way along a path new to his generation of prominent legal scholars.

The Warren Court rushes toward an "egalitarian society," he argued. Its two central efforts seek to "nationalize and level" the qualifications for voting, and to extend desegregation from removal of segregatory laws to racial balancing. It aspired

> to a groupless society, to a politics without faction, and a theoretic preference, as Madison said, for "reducing mankind to a perfect equality in their political rights"–a preference sustained by a prophetic judgment that the United States is now, or soon will be, so nearly classless a society that Madison's analysis is dated (1970, p. 174).

This universalistic society of the future is to be homogeneous: groupless and majoritarian (1970, p. 103). It is also to be "legalistic" and "centralized." Judges (not least of the lower federal courts) have come to display an "imperfectly bridled managerial drive," which increasingly assimilates private action to the governmental action reachable under the Equal Protection clause, and increasingly overthrows "jurisdictional fetters" (1970, p. 104 ff.). Matters once largely under local and state police power, such as regulation of obscenity and of criminal procedure, are increasingly governed by federal judges, judges who often see themselves possessed of "roving commissions" to rectify evils (1970, p. 134).

Although Bickel still worries that the Court's unpopular "principledness" threatens its own standing, and that the "heady tide" of majoritarianism it loosed may not spare itself, he now questions chiefly the substantive impact of Court on country. Might centralized, egalitarian, legalitarian government bear the "seed of tyranny" (1970, p. 115)? Are quality of life and community of life threatened by a national homogeneous egalitarianism suspicious of all boundaries and distinctions separating people? Bickel draws support from a well-known remark of Brandeis:

> For a century our growth has come through national expansion and the increase of the functions of the Federal Government. The growth of the future–at least of the immediate future–must be in quality and spiritual value. And that can come only through the concentrated, intensified striving of smaller groups. The field of special effort shall now be the State, the City, the village–and each should be led to seek to excel in something peculiar to it (1970, p. 116).

In arguments that have provoked sharp controversy not least because there is now reason to think them prophetic, Bickel insists that the Court's policies of racial balancing and one man, one vote threaten distinctive neighborhood schools and genuine communities. Such politics may even provoke the opposite of what the Court seeks, as white flight in many cities threatens equality of education, quality of education, and integrated schools, and a proliferation of gerrymanders subtracts by manipulation from any additions in participation. Indeed, should "the future" go in Brandeis' direction (as Bickel's residue of historical faith puts it), these decisions will be irrelevant or even obsolescent (1970, pp. 151, 173, 137–38).

Bickel's recent and last book, *The Morality of Consent,* turns from spotty if original critique to deeper critique and to reconstruction. *The Least Dangerous Branch* had celebrated politic judicial techniques for applying a vaguely equalizing principle. *The Supreme Court and the Idea of Progress* cautioned against judicial application, by novel techniques, of an egalitarian faith now seen itself to be impolitic. *The Morality of Consent* elaborates Bickel's turn from the techniques of judicial power to the wise direction of judicial power.

Bickel has now proceeded beyond the complacent supposition that still informed *The Supreme Court and the Idea of Progress*, that "society" at least would "develop" on its own should his plan to limit courts be adopted. The self-regulating pluralism criticized by Judge Wright has disappeared (Wright, 1971, p. 783): instead Bickel inquires as to what sort of practical public regulations need be devised to protect both our liberties and our institutions. He also moves beyond a narrow preoccupation with courts, which had followed from Holmes' apolitical premise that judges in particular discern the path to the future. Correspondingly, he goes beyond his earlier books' loose description of judicial "faith" to confront the actual tenets of contemporary liberalism in general, not least in the universities and among journalists; he focuses more upon definite liberties, not merely alleging a vague egalitarianism. He tries to correct, for example, certain corrosive effects of the right of free speech as reinterpreted by recent courts to include "self-expression." We find practical and serious discussions, infused with political urgency, on the meaning of free speech in a country like ours, on the law of obscenity and of the news reporter's privilege, on conscientious objection and civil disobedience, and on the proper aims and duties of intellectuals and of universities.

In his new political seriousness Bickel tolerates less of not only his own old optimism but also of the "catechism of principles" that he fears. The conclusion of *Supreme Court and the Idea of Progress* had been called "Remembering the Future," and there Bickel had allowed that the "future may yet belong to the Warren Court." Its liberalism was not "an unworthy ideal" and might even be *the* liberalism of our tradition (1970, pp. 173–74). This is elegantly confused futurology; in the cold light of political and moral appraisal Bickel is now more severe. The tenor of *The Morality of Consent* is foreboding, sober with impending danger. Bickel portrays a free order under siege and ill-defended. There are, of course, other such portrayals, the most famous being Alexander Solzhenitsyn's. Bickel's is not quite Solzhenitsyn's alarm bell in the night, meant to reawaken sleepy democracies to a foreign menace. With exceptions to be later noted, Bickel's work hardly touches upon our international situation. Solzhenitsyn himself, however, had found a cause of apprehension in a "spiritual crisis" of the democracies themselves; their leading circles are awkwardly reluctant to judge of "good and evil" and to stand up for their own ways of life (Solzhenitsyn, 1975, pp. 8, 22–23, 30). While Bickel also seeks to awaken his readers, it is to this sort of siege from within, directed against the regime we have known, and armed chiefly with ideas.

Supreme Court and the Idea of Progress had displayed a tentative but revealing self-doubt about the validity of its own arguments. Brandeis' program of decentralization had been set forth as but an alternative "future," and Bickel's own political "ultimates"—certain reservations about simple majoritarianism and egalitarianism—as but a parenthesis "revelatory" of his opinion (not as wise considerations upon which to act) (1970, pp. 114–17, 173–74). This self-destructive deference to history's judgments and to subjectivity of judgment is absent from *The Morality of Consent.* Indeed, such doubts are now portrayed as part of the very crisis besetting liberalism, a part corroding liberals' confidence and energy in political reflection and action. One part of the problem of moderate liberalism is that liberal arrangements are often neither thoughtfully interpreted by vigorous people nor vigorously defended by thoughtful people. After all, these doubts corrode the determination of thoughtful people especially, the only ones who take very seriously the alleged authority of historical ages and scientific methods. "What is the use of empty 'rationalists,' such as were discovered at many a university some years ago, who being confronted with various demands for instant change, found that they believed nothing and could not judge any change as better or worse?" (1975, pp. 24–25)

All of Bickel's works are shadowed by modern historicism and positivism, by the shadow of "value relativity." Even *The Morality of Consent* tends to exaggerate the variety of individual "values," to hesitate in judging of them, and to suppose still that principles "evolve." Yet Bickel is revolted by the attempt to abstain from choice; that way lies "moral suicide" (1975, p. 77). This book allows to Bickel's moral and political perceptions and reasonings an express priority over the artificially amoral and apolitical canons of scientific and historical jurisprudence, canons that infiltrated his earlier books more. His reasoning seems more practical and philosophic, in an old sense, and less objective or historical or even theoretical, in the common contemporary senses of these terms.

We can now understand, with provisional sympathy at least, the startling if sketchy beginning of *The Morality of Consent.* It critically reviews the tradition of political theory that has influenced the United States, and in particular it returns to the political thought of Edmund Burke as a touchstone. In this, his most theoretical book, whose theme is the danger of modern theories for our law, Bickel turns to the theorist Burke, whose distinctive theme was the danger of modern theories for practical life. When a leading American constitutional lawyer and thinker returns open-eyed to political philosophy, and to Burke in particular, that is news. It is true that there is much of Bickel's "evolution" and "values" and "process" in his interpretation of Burke. It is more interesting for present purposes that Bickel felt the need to deepen his understanding with Burke's, and to deepen constitutional interpretation in a way usually forgotten where not ridiculed as "reactionary."

Why the odor commonly attending Burke's memory? Educated by Holmes and Mill and Marx and Kant to believe in a social progress that entailed the progress of humanity itself, heedless therefore of the fragile conditions of a free society, intellectuals forgot Burke's problem. They thought they could forget Burke. Provoked by zealous moralism and despairing relativism, Bickel has rediscovered Burke and with his aid hopes to avoid the "opposite evils" of "intolerance and indifference" (1975, p. 12). In the face of the theoretical politics of the French Revolution, Burke restored respect for the "practical wisdom" of a statesman, attuned to the obvious arrangements and goods and limitations of a particular polity. Bickel distinguishes the sober Burkean tradition in our politics from the "contractarian" view of Locke and Rousseau. The contractarians measure any particular society by universal, pre-social, natural rights whose "premises ... cannot be questioned closely." The Burkean tradition, however, "begins not with theoretical rights but with a real society" and with "human nature as it is seen to be" (1975, p. 4). In as radical a surgery as has been performed on the American political tradition since the fall of the Confederacy, Bickel would cut the contractarians off. Recalling Burke's Whiggism, he even disputes their exclusive claim to be "liberal." Bickel points beyond not only scholarly historicism, but also the conventional political distinction, post-Wilson, Mill and Kant, between the liberal party of progress in universal self-determination and the conservative party of reaction.

Bickel has not simply recurred to some eighteenth-century regime, however, nor to the confident treatises of a Story or a Cooley, nor to the pre-New Deal Constitution of fixed rights and powers and free enterprise. He is painfully aware that his country participates in both the old Enlightenment and the new doubts about that "Age of Reason." Besides, precisely Burke's "method of reason" raises doubts about pat solutions, and requires respect (excessive respect, I must say) for present conditions. "Circumstance is all," says Bickel, and the rights and powers now established in welfare capitalism differ from those once protected. Moreover, the residue of Bickel's faith in progress still supposes that our principles, at least, have "evolved." For all these reasons he focuses upon the sorts of speech and press now given special priority by the Court (and by the revised liberalism derivative from Mill and Holmes). He considers how to define, permit and limit these modern concerns in order that they, together with the whole "legal process" upon which they depend, may be preserved.

Loosely put, Bickel accepts as a starting point a number of the contemporary aspirations of what may still be called New Deal liberalism. He tries, however, to think out a reasonable political provision to replace an illusory faith in historical provision, a faith that breeds zeal and also complacency as its adherents concentrate upon providing ever more free speech and ever more security for the little man (1975, pp. 25–27).

Within these distinct confines, Bickel's work may be the most important consideration of the American Constitution since Holmes replaced fixed private rights and public powers with a developing law kept up-to-date by the judicial process, itself attuned to a marketplace of ideas ensuring the progress of society. Alive with political reflection, free of obeisance to either conventional liberalism or conventional conservatism, *The Morality of Consent* advances beyond the indecisive formalism besetting Frankfurter's "balancing" according to the "needs of society," Wechsler's "neutral principles," and Bickel's own earlier works.

Liberalism without Liberty

All this is not to say that Bickel's last formulation is free from difficulty. To repeat his diagnosis: liberal democracy is under siege, most obviously from a militant but destructive liberal individualism; Bickel suggests we turn from the "ideas" of liberal individualism and seek agreement upon what may be called liberal constitutionalism. Both are liberal. Whereas individualist democracy tends to destroy itself by its permissive and egalitarian ways, however, constitutional democracy prudently accommodates the mores and hierarchies that sustain it.

A difficulty comes to light. Can one preserve a liberal constitution without caring for the liberties that make it liberal? Suspicious of a politics of "ideas," following Burke's attack on a politics of the rights of man, Bickel determines to encourage agreement upon means alone. The "morality of consent is a morality of process," of "legal process" in particular. He will avoid discussion of "evolving" ends (upon which we are said to agree), for he asserts our differences to be of "policy," of means. What then distinguishes our liberal legal process from an illiberal legal process, or makes us insist upon a process governed by law? Bickel himself observes: "A limited number of broad first principles concerning the ends of government ... [is] what the law of the Constitution is about" (1975, p. 142). Indeed, Bickel's own morality of consent seems an unacknowledged inference from the old supposition that individuals are naturally free and not to be governed except by their own agreement.

"The moral authority of liberalism is lost when the fundamental principles and attitudes of liberalism are compromised or abandoned, for these principles are the essence of the American political tradition.... They are the foundation of the liberal ascendency in our politics" (1975, p. 139). So Bickel remarks on the consequences of "disoriented liberals and intellectuals" who cater to radicals farther left. Is Bickel guilty of a similar disorientation, albeit toward the right? Although he insists that as good Burkeans we cultivate the particular arrangements of our country, he also insists that we ignore the principles that in one form or another have hallowed and guided our arrangements. Are we not to "hold these truths to be self-evident, that all men are created equal, that they are endowed by their creator with certain inalienable rights; that among these are life, liberty and the pursuit of happiness"?

This seems an unpromising kind of surgery, that extracts the heart with positive relief at the absence of a like organ for transplant. Can a step of such gravity be necessary now, not even to raise the question as to whether such a revolution is possible now? One can say at least that Bickel does not prove its necessity. In particular, his indictment seems to apply less to the country's liberal tradition than to a new kind of liberalism whose extreme wing was the New Left. In *The Morality of Consent* Bickel's chief object of attack is the individual liberation and social egalitarianism promoted by the "movement" of "the Sixties" and, in a moderated form, by segments of the Warren Court. He seems to be attacking not the practices that follow from traditional rights to life, liberty, and pursuit of happiness, but those that follow from newly coined rights to privacy, self-expression, and equal dignity. Sensitive and committed selves, it is said, ought feel their country's guilt in the systematic repression of the disadvantaged at home and abroad, and ought commit themselves to a new liberation from the old establishment—a liberation of self-expression from the old morals, and of equal dignity from repressive structures ranging from the hierarchical family to the economy and the universities.

On this point we part company with Judge J. Skelly Wright's criticism of Bickel's earlier work. With reason Judge Wright remarks that Bickel's process-oriented jurisprudence fails to provide for "our constitutional rights and liberties" (Wright, 1971, pp. 784, 779). "The most important value choices," he replies to Bickel's skepticism, "have already been made by the framers of the Constitution." Let us agree with Wright's remarks, as befitting an American judge. And yet there seems some ground for Bickel's skepticism of modern judicial subjectivity, if we examine further Judge Wright's own views. For the judge does not show and could not show that the framers of the Constitution (or of the Bill of Rights or the Fourteenth Amendment) had in mind the rights or "ideals" which he himself seems to have in mind. He mentions, for example, rights to individual "dignity" and "privacy," the "ideal of equality of political opportunity" ("every citizen should have a roughly equivalent opportunity of access to the formal levers of political power"), and "certain minority rights" understood chiefly or merely as the rights of "disadvantaged minorities" (Wright, 1971, pp. 800, 788, 794). Judge Wright concludes a spirited review by celebrating and encouraging the Warren Court's "revolutionary influence on the thinking of law students" (Wright, 1971, p. 804).

Another critic of Bickel, Edward Purcell, has written that *The Morality of Consent* fails to confront the "intellectual challenges" of the 1960s—the challenges to "objectivity," to the power of "elites," and to the authority of reason itself (Purcell, 1976, pp. 562–63). There is truth in this, and yet it slights the massive political and moral commentary of Bickel's last book.

The indictment leveled by the New Left, he thought, was so massive as to be politically irrational. We are still quite obviously a "free society" and not a "police state." Besides, "it is not reasonable to extend a systemic indictment to the entire structure of government, to the electoral process itself, to the administration of justice, to every debatable action that a new national administration thinks it has a mandate to take, and to every type of institution.... Destructive nihilism is evil no matter how motivated" (1975, p. 137).

"Nihilism" is first of all an "ism," the sort of secular creed that seems to motivate modern political movements. As nihilism insists that nothing is good, that all culture hitherto, for example, has been repressive, its "destructive" form insists that the things established must be destroyed. To return to Bickel's account, "the movement" harped on a "systemic crisis," with "incredibly loose talk" about the "obsolescence and rottenness of our society and all our institutions" (1975, p. 137). The sixties, he said, saw a kind of total war against everything in authority, against "the establishment." It was a war conducted almost without moderation and without reason itself. Action without purpose and for its own sake became a cause: "activism." Bickel was struck by an absence of positive goals and the prevalence of hate, self-despising, and contempt for reason. The militants' rhetoric abandoned all pretense of allegiance; it acknowledged no restraint and no bounds. Just this destructive irrationalism is *the* danger, however. The true crisis of our time "is the crisis of the abandonment of reason, of standards, of measure, the loss of balance and judgment" (1975, p. 137).

At this point the course of Bickel's diagnosis almost visibly breaks through his original formulation. More than its own excesses besets the Age of Reason. It reels under a theoretical attack upon calculating or technological reason in the name of a more profound or expressive self. Precisely the "systems" and science and progress so prized by the Enlightenment are suspect, suspected of poisoning a whole "culture" and indeed the history of culture hitherto. The attack was prefigured in the reflections of Rousseau and of Marx. It became thematic in Nietzsche's later and more radical cultural critique, now reformulated by Heidegger and the ensuing existentialists. *To be* is to be authentic, concerned, to be one's own "self." This gave the distinctive flavor to the crude American ideologies of the sixties and seventies; it caused the "imperialism" of concepts like freedom that take on the aura of liberation (1975, p. 11). There follows an unsparing and total critique of the commonness of bourgeois mores and "systems" of government and methods of inquiry.

The philosophers of unreason seem surprisingly untroubled by their foundation in philosophy and reason, one might note, or by an attack upon distinctions and qualities that itself distinguishes an authentic from a hypocritical self. There are difficulties when subtle reasoners endeavor to evict practical judgment from human nature. The attempt seems to have led not only to self-contradiction in principle, but also toward a practical encouragement in conduct of zealotry, of licentiousness, and above all of aimlessness nevertheless aiming for "meaning."

Whatever the defects intrinsic to our order, the particular siege Bickel describes is conducted with imported ideas. True, our original mixture of individualism and calculated reliance on artificial institutions has its own corrosive effect upon custom and community. Yet the framers and Jefferson and Lincoln, among others, supplied also some account of civic education and moral guidance. True, the later "First Amendment" liberalism has destroyed many of the materials for such civic and moral and religious education. Yet this was partly redeemed by its compassionate social idealism. In short, one might think that the problems attending our original liberalism, and its Wilsonian and New Deal variations, only make the country peculiarly susceptible to this latest development of modern philosophy. Bickel misses the important point: the contemporary dispute is over ends. Bickel would substitute the "computing principle" and "accommodation" for moral imperatives and the clash of ultimates (1975, pp. 19, 24, 30). However politic in ordinary politics, such a course is insufficient for what de Tocqueville called great politics, when principle is in dispute—especially when the dispute occurs in a liberal order *founded on self-evident truths*. A liberal order cannot accommodate illiberal ultimates: the right of property in slaves, for example, or the right defended by Nietzsche, to express oneself by torturing others. Liberal constitutionalism cannot be moderately restored without a moderate account of the liberties that the Constitution protects.

Indeed, Bickel's own practical discussions of this country's sort of rights and laws bely somewhat the self-denying ordinance of his theory. Many of the materials of the old liberal faith are evoked by his instructive discussions of a free press, civil disobedience, and so forth. Perhaps his most telling remarks concern "affirmative action" programs for recruiting minority faculty and students. He had become doubtful of such programs, a change from *The Least Dangerous Branch*'s defense of "benevolent quotas" (1962, pp. 60–65, 71; 1975, pp. 131–32). He still thought we should recruit more, tutor more, use only relevant tests and standards, test only capacity and not irrelevant culture. "Instead, we have often yielded to pressures simply to relax standards" (1975, p. 132). An egalitarian pressing of sympathy for the "disadvantaged" beyond individual deserts, he thought, is simply unjust in favoring those less fit, depresses "efficiency and productivity" in crafts, professions, and business, and derogates from the dignity of those patronized. To dilute standards of accomplishment, to prefer the less qualified to the more, "is morally wrong," Bickel contends.

> And in a society in which men expect to succeed by hard work and to better themselves by making themselves better, a society, moreover, in which prejudice for some groups has only recently been overcome so that the expectation has begun to be fully met, it is no trivial moral wrong to proceed systematically to defeat it (1975, p. 133).

Here we can see Bickel expand his diagnosis of the new egalitarianism to confront its danger to a sort of justice, to an end that has crucially shaped our society. He does not attend to or account for the "expectation" he notes, however. What clarifies that old faith, still widely believed although now less so, that our citizens may "succeed by hard work and better themselves by making themselves better"?

Bickel had spoken generally of the need for "those visions of good and evil a society draws from its past;" without these, "it dies." In particular we need to recur to "the secular religion of the American republic" (1975, p. 24). Bickel might have profited in his last book, as he had in *The Least Dangerous Branch*, by returning to the most famous and articulate statesman of the country's equal rights tradition, Abraham Lincoln. Meditating often on these subjects, Lincoln had nevertheless a simple account. The essence of our old faith, he thought, is the proposition that all men are created equal in their rights to life, to liberty, and—here relevant—to pursue happiness. Why, Lincoln asked rhetorically as he clarified for Congress and the North what the Civil War was about, why did the Confederacy

> omit [from their declaration] the words "all men are created equal"? Why this deliberate pressing out of view the rights of men, and the authority of the people?
>
> This is essentially a People's contest. On the side of the Union, it is a struggle for maintaining in the world, that form, and substance, of government, whose leading object is, to elevate the condition of men—to lift artificial weights from all shoulders—and to clear the paths of laudable pursuit for all—to afford all, an unfettered start, and a fair chance in the race of life (Lincoln, 1861, p. 438).

Lincoln interprets the equal rights tradition moderately, in a manner neither permissive nor egalitarian. It is not permissive because the pursuits authorized are "laudable," praiseworthy and respectable. No special warrant is given to the merely private or self-expressive. Also, the sort of equality to be respected is limited to that of certain rights, of opportunity, for example, including (but not limited to) opportunity for economic advancement. Besides equality of opportunity, there is to be opportunity for inequality, not least in quality of accomplishment. Nothing was suggested like a right to be "recognized" as possessing "equal dignity." While a common basic dignity may be conceded to all by virtue of common natural rights, the distinctive dignity of any person depends upon his or her accomplishments, the particular use each makes of his or her opportunity. According to this understanding, for example, there is dignity in labor. Neither human dignity nor human rights, therefore, ought be separated from what Bickel tends to call "moral values," from qualities of character that by earlier statesmen like Jefferson and Marshall were still called republican virtue. In Bickel's admirable phrase, men are "to better themselves by making themselves better" (1975, p. 133).

Charles Frankel put the point quite well some years ago while comparing the traditional American political creed with the new egalitarianism. The traditional teaching was not loosened and made self-contradictory by a relativistic refusal to judge of merit, nor was it forced to judge of someone's merit by the vulgar standard of success.

> Behind the roles that men performed and the positions they occupied, it saw them in terms of their general traits of character.... The virtues it admired were drawn from the traditions and experiences of all classes. And while it espoused equality, it did so in recognition of the value of other things which create differences, partisan feelings, and stratifications in society—parental and personal affections, voluntary association and friendship, the desire of people to join with others with common experiences and tastes, and, not least, the need in every society to give public recognition to things noble and excellent lest everything in the society's culture be regarded as disposable. These are all considerations that set some limits upon the proper range of the principle of equality. They do not subvert it; they merely keep it sane (Frankel, 1973, p. 61).

If Lincoln's is a reasonable way to understand the American creed at its best—and this is not the place for an extended argument—then Bickel's effort to cut liberty from liberalism is unnecessary as well as paradoxical.

The effects of the effort are surely paradoxical. One of the strangest arguments Bickel ever made occurs in his second chapter when he tries to excise from the Constitution any mention or notion of "citizenship." With truth he says that the framers were concerned with "persons" or "the people." He argues, however, that they did not also provide special rights and special duties for members of this *political community*. In turn, Bickel must explain away the Constitution's various references to "naturalization," to citizenship as a condition of political office, and to "privileges and immunities of citizens." He even feels a strange need to equate Chief Justice Taney's twisted arguments for enslaving Dred Scott with Chief Justice Warren's efforts to loosen restrictions on alien entry and to impose them on Congress' power to expatriate (1975, pp. 33–54). Oddly, the Constitution so construed would encourage only an apolitical and asocial individual, stripped of the rights and duties that pertain to "real society" and of the political virtues needed for a citizen of a free society. Bickel ends by advancing just the sort of theory Burke had thought "not fit to live with," the sort that leads to a "savage and brutal" humanity (1975, pp. 23, 13).

Bickel seemed to be trying to avoid certain totalitarian implications he feared in contractarian theories of citizenship, especially judicial theories. If justice originates only from a contract of membership in society, then interpreters of that fundamental bond may control all, even all "social and economic matters." The American way differs, he says; even aliens are protected in their "right of occupation," and by Court decisions of long ago.

Bickel is misled by the residue of his historicist theory of evolving principle. The consequence of this confusion in theory is imprudence in practical advice. He confuses the older natural rights theory with Rousseau's very different "democratic theory," of which general will, equal dignity, and democratic citizenship are characteristic. More to the point, he ignores the provision by Rousseau and the framers alike for republican citizens no less than free men. From the natural right to pursue happiness comes the "civil right" to an occupation. Yet a free republic must look to civic rights and civic duties as least as much as to civil rights. Bickel simply neglects here the connection between free institutions and suitable citizens. He can't *imagine* why Chief Justice Stone could have argued that Congress can require from "prospective citizens" allegiance to such principles as "protection of civil rights and of life, liberty and property, the principle of representative government, and the principle that constitutional laws are not to be broken down by planned disobedience" (1975, p. 49, quoting *Schneiderman* v. *U.S.*, 320 U.S. at 181 [1943]). He is surprised, moreover, to find the un-Burkean theory he fears beneath the opinion he adopts. "Citizenship *is* man's basic right for it is nothing less than the right to have rights," said Chief Justice Warren, arguing against a congressional power to expatriate naturalized citizens who had failed to fulfill certain stipulated conditions. "Remove this priceless possession and there remains a stateless person, disgraced and degraded in the eyes of his countrymen" (*Perez* v. *Brownell*, 356 U.S. 44, at 64–65 [1957], quoted in 1975, p. 52). In such circumstances prudent concern for good citizens comes to seem mean and illegitimate to generous people, in the flush of a devotion to the equal dignity of all.

Bickel's attempt to moderate political zealotry by excising liberty from liberalism leads, we conclude, up a blind alley where few judges or public officials worthy of our heritage will follow. Encouraging officials to disown our rights, Bickel fails to encourage their protection in a reasonable form. Attempting to eliminate the Constitution's obvious acknowledgement of citizenship, Bickel fails to encourage the good citizenship slighted by the abstract humanitarianism he fears. A like difficulty afflicts his reinterpretation of the remainder of our constitutionalism, the institutions of popular government.

Constitutionalism without Government

Our liberal democracy seems a mixture of liberalism and democracy, a variation upon the framers' original liberalism and original republicanism. Their understanding embraced constitutionalism on the one hand, vigorous governmental powers fixed by fundamental law, and representative democracy on the other. Bickel's formulae vary the ingredients and thus the mixture. His general notions of "legal process," "consent," and "evolving principles" tend to hollow out liberal and popular arrangements into something merely procedural. Self-government and government alike turn into a nebulous acquiescence to a nebulous process of inquiry with the object of evolving nebulous principles. This is a gross oversimplification, but not too gross. Representative government is replaced by consent to a process (albeit an "active consent"); rights and powers fixed by law are replaced by a legal process developing first principles. Bickel's slighting of citizenship is a sign of his distance from the framers' democratic republicanism. In place of the civic qualities and institutions needed for political self-government, Bickel elaborates a morality—a canon of individual conduct—and a morality of consent in particular—a canon of law-abidingness or, more precisely, of tolerance of the legal system. Bickel focuses not on shared common arrangements, but on individual restraint; not on the activity or structure of governing, but on acquiescence to some initiative from without. *The* political problem, it seems, is not who governs or how to govern well, but how to induce toleration. Here enters Bickel's supposition of an infinite diversity of "values." The problem for Bickel: How does one get individuals with diverse "values" to tolerate the system—and the system to tolerate a moderate diversity? Thus the need for a morality of consent, shared by those in office and without. There must be a governing morality, at least, a common loyalty to our arrangements. That is Bickel's reply to libertarians, to egalitarians zealous for ever more universal participation, and to social scientists preoccupied with systems of power.

Whatever Bickel's merits compared to his contemporaries, his morality of consent is not without difficulties of its own. This morality seems abstract and legalistic; Bickel talks much of complicated consent, almost never of simple patriotism or simple idealism. At book's end he moves feelingly to evoke the "mystic chords of memory" with which Lincoln sought to recover the South's loyalty; Bickel's celebration drifts off into evolutionary indefiniteness. Also, his "morality of consent" amounts to hardly more than a generalized loyalty or law-abidingness, important to a good citizen and fair person but hardly the whole of citizenship or virtue. This morality seems pedestrian, unduly subservient to the contemporary and decayed liberal democratic ethos, and passive and private in its focus on acquiescence. It is no accident that Bickel slights the development of leaders no less than of ordinary citizens.

Bickel's outlook deprecates not only a political disposition but also political institutions themselves. Perhaps moved by his experience of Watergate, he does insist upon an "absolute duty" to follow the "manifest constitution." This is the "constitution of the mechanics of institutional arrangements and of the political process, of power allocation and the division of powers, and the historically defined hard core of procedural provisions" (1975, p. 29). To disobey is to deny constitutionalism, "a set of pre-existing rules within which society works out all its other rules" (1975, p. 30). Yet Bickel dilutes this premise of constitutional democracy as he reaffirms it. His examples are of very "mechanical" provisions and only of limits on government. The real powers of government and the real discretion of interpreters turn out to involve the Constitution of "open texture and evolving principle." Marshall "spoke as if most of it were manifest"; modern judges do the opposite (1975, pp. 29–30). Bickel follows modern judges. He tends to replace the fixed rights and powers of constitutionalism not with other definite powers, but with an indefinite legal-intellectual colloquy by which principles (and thus principled powers) are evolved. Citizens and statesmen may merely "respond" (and not govern) because political initiative is still provided by the residue, in Bickel's jurisprudence, of History. The process seems to involve, first, a kind of judicial accommodation of the useful claims of conscience and civil disobedience, second, a free press elevated to the status of "balance" of government, and third, moderate intellectuals, especially in the universities, who will uphold the liberalism of process while "free inquiry" proceeds. As in *The Supreme Court and the Idea of Progress,* Bickel seems to yield final authority for ultimate principles, if such do exist for him, to "moral philosophy and political theory" (1970, p. 86).

It seems that judges, journalists, and finally intellectuals are to govern. Still, this process of consenting is hardly governing: it is more "communicating" or "educational" conversing (1975, pp. 67, 111). At one point Bickel suggests that enforcement denotes the breakdown of the legal order (1975, p. 112). Strange; authoritative enforcement seems a part of any legal order. Bickel would replace governing by agreeing. In this book about "legal process" there is scarcely a word about police, except a remark that freedom is in danger where policy needs to be enforced by force (1975, pp. 106–07). There is scarcely a word about armies. Bickel wisely says that "the waging of war needs continuous political support," and that a democracy "should not wage a war which a substantial and intense body of opinion resolutely opposes on both political and moral grounds" (1975, pp. 102–03). And yet: suppose an unpopular war needs to be waged? France in 1939, a democracy divided, needed to war with Nazi Germany to remain free. True, one may grant that a society riven makes government impossible, and that extreme governments may divide society, not least by foolish wars. What, however, should government do and be when governing is possible, and how can one best avoid a foolish or extreme government? Such questions led the Philadelphia convention to establish institutions, especially an executive, that might mix foresight and forcefulness with the habitual shortsightedness and sluggishness and impulsiveness of the democratic sort of republic the convention also favored. The first need was a government that could govern; the second, mutual checks for the security of those governed. Bickel retains checks and mixes in arrangements for political disputatiousness. He seems to leave out adequate arrangements for governing.

Bickel's deference to an indefinite process affects his understanding of the limits to government as well as the powers of government. Originally the country was to have national institutions limited in two senses, limited in powers, limited by a system of federalism. That times have changed and the needs of the country have changed, of course, is a saying not only old but true. Yet this is not to say (with Frankfurter) that change is the law of life, as Bickel continues to say. What is now needed is reflection, in light of present circumstances and yet unafflicted by preconceptions as to the desirability of "social change," on the old limits and old divisions of authority. Bickel knew of the problem, needless to say. *The Supreme Court and the Idea of Progress* had suggested a renewed cultivation of state, city, village, for the sake of quality of life and of community. But *The Morality of Consent* accepts too easily the constitution of "open texture," traipsing abjectly after an open and indefinite historical process. Bickel's thoughts tend to be abstracted from the corporations and unions and technology and cities and classes of this particular country, and from the practical purposes and necessities and institutions of its governments. His thought presupposes the country's arrangements while abstracting from them. This is another part of the problem of moderate liberal jurisprudence. Bickel inherited his optimistic indefiniteness, and it is only superceded (not eliminated) by the political directness and seriousness of his last book.

Zeal and Defection: The Practical Problem

Of revolutionary France, Burke had said: "a silent revolution in the moral world preceded the political and prepared it" (Burke, 1826, Vol. 8, p. 259). Bickel feared a like connection in his own country.

He concludes *The Morality of Consent* by warning against a revolution and tyranny of the Left (1975, pp. 139–41). Earlier he wonders whether any "other society could have endured without a change of regime" the several waves of civil disobedience (by white southerners and by civil rights and antiwar activists) of the past 15 years (1975, p. 112). Bickel's is thus no conventional warning against increasing lawlessness. He does not mention crime in the streets or violence in the schools or white-collar crime. He has a more subtle lawlessness in mind (although it is an impolitic subtlety that filters out obvious sores). Law is not a set of rules, but a "process," we are told, and it is the whole process that is endangered. This process is our constitutionalism of fundamental law and fixed institutions, the attitudes and values that make them work, and especially the shared ideas—"the moral authority of liberalism"—that guide attitudes and values. Legal process is especially the ideas that guide legal development, and these are at once under siege and ill-defended. Many officers of the defending force have lost heart for any struggle—those "empty rationalists" who stand for nothing—and many others have half-defected in their heart of hearts: "Liberals who want no revolution are forever trying to appease the revolutionaries in order to entice them back up on the raft" (1975, p. 139). The battle is in the hearts of liberal intellectuals most of all, according to Bickel, and *the* problem of liberal democracy now is a problem of morale. Bickel knew of a certain calm or apathy returning to American campuses. There remains a crisis of the spirit. One sees a loss of direction or even a radical shift of direction, especially among intellectuals and those, on the bench and in the press corps, educated by them.

In American circumstances, according to Bickel, the new irrationalism appears in a blind "commitment" without limit to the "moral imperatives" of equality and liberty, and a blind attack upon the old limits. The limits thus ignored and attacked may even be those needful for serious equality and liberty. The danger, Bickel says, of the "revolutionary frame of mind" is not only that

> it may evoke an effective counter-revolution; it is dangerous also, the more dangerous, because guilt-ridden and otherwise disoriented liberals and intellectuals are intimidated by it into damaging the institutions that hold out the only realistic hope for the redress of many legitimate grievances, and that alone stand in the way of counter-revolution (1975, p. 138).

Bickel muses on the "movements" for ever more universal equality of voting rights and ever more freedom of speech. These form a strangely self-defeating combination. "Only in the smallest kind of city-state constituency" can a majority actually control. Modern liberals insist, however, upon "enlargement of the constituency" for the sake of a variety of expressions, a sufficient marketplace of ideas. Alternatively, among "mass audiences" it becomes more difficult "to assume that they can be reasoned with, and to assume that in the marketplace of ideas truth will drive out error and good counsels prevail over bad" (1975, p. 7). And yet those devoted to "egalitarianism, popular sovereignty, and free speech" are positively suspicious of local and state discretion. There develops a mass electorate without opportunity for serious experience of self-government, and a mass marketplace of ideas not conducive to serious discussion and education. A mass electorate tends to foster a plebiscitary executive, a mass "audience," the politics of manipulative imagery.

Bickel's portrayal may be compared with de Tocqueville's. Even 150 years ago the French observer foresaw the danger of a manufacturing oligarchy. Yet he also doubted its lasting predominance in the face of the American majority's political power. De Tocqueville feared more a new sort of political "despotism" singularly fitted to democratic nations and their representative and centralized governments. He feared a mass society increasingly passive in mind and spirit after its fundamental control over government had been established, increasingly willing to subject itself to administrative providers all-solicitous, but also all-controlling (de Tocqueville, 1969, Vol. 2, Pt. 4, Ch. 6). What Bickel portrays, however, is somewhat different.

De Tocqueville had thought the chief force would be democrats' passion for "equality," and the chief villains, ordinary people. A democratic citizenry would willingly augment the centralized administration of a modern government regarded as its agent or servant, thus to obtain the comfort and security promised as its right without either the pain of labor demanded as its duty, or the pain of deference to superior individuals. According to Bickel, however, the chief problem lies less in common desires and envy than in liberal intellectuals' insistence (a modern force for equal rights slighted by *Democracy in America*). Contemporary intellectuals insist indeed upon provision of "human welfare," often with a distinct contempt for the work ethic. They insist principally, however, upon an equality free from judgments of quality, a justice without "discrimination" as to moral quality and with "liberation" from established mores and distinctions. According to this account, the path to the destruction of liberal democracy is less a direct road via the welfare state to organized institutional control. It is also and more an indirect road, via corrosion of distinctive communities, of moral and religious restraints and of respect for law, to a more irrational and personal national leader, likely to be of the self-righteous Left or of a resentful Right.

Bickel is fond of pointing to the authoritarian implications of the new "movement" itself. Like the modern court, it espouses equality and liberty and yet insists that a "catechism of principles" should prevail as the general will, whether or not any actual majority freely accepts. As Bickel examines conditions in the universities, indeed, he waxes more apprehensive about a dictatorship of the self-righteous. Yet he believes dictatorship will more likely descend from a force other than the intellectuals.

Watergate, Bickel thinks, portended what is to come, and repeatedly he meditates upon Richard Nixon's excesses and fall. He traces the excesses to a combination of active zeal on the right and reaction to zeal on the left, all combining to weaken customary restraints upon executive power. Checks and balances worked eventually, but many lines of defense had been breached. Watergate symbolizes the "weakened capacity" of our "legal order" to guard against "self-righteous abuse of executive power in the service of ideological or moral ends" (1975, p. 93). Those who appealed to the silent majority had their own morality of clean living, patriotism, law and order. These ends could have taken the president farther outside the customary constitution, the morality of process as Bickel calls it, than did the abuses of Watergate proper. Soon after the election of 1972, Bickel recalls, "Mr. Nixon gave the impression that he thought the American political process had taken place, so to speak, that it was over for a while and that he could simply rule" (1975, p. 122).

Yet the causes were not simply in Nixon, although they were chiefly there; they were also to be found among the intellectuals. Bickel rejects any simple Coffin-to-Magruder-to-Nixon link. Nevertheless, that "moral firestorm" of the sixties did break down customary law-abidingness and political moderation among the politically active (1975, p. 93). Virulently undiscriminating attacks on a "fascist" administration fighting an "imperialistic" war did extend to leaks of official secrets, and to breaches of trust, that gave a precedent and a certain justification for the administration's own excesses. Above all, Bickel is impressed with the indirect consequences of the new populism. It had derogated procedures and institutions and law. It had celebrated "one-person, one-vote": a suffrage stripped of prudent qualifications, such as connection with locality, due regard for interest and education, and the need to balance institutions and encourage consent. The egalitarian spirit itself leads to "concentration of power in that single institution which has the most immediate link to the largest constituency." For all these reasons a Gaullist presidency is encouraged, of "inherent powers, futurism, populism, and certainly moral urgency." "I don't know when Mr. Nixon caught the liberals bathing," writes Bickel, "but he did walk off with their clothes and stood forth wearing the plebiscitary presidency, his own futurism, and his own moral imperatives" (1975, pp. 121–22).

Practical Remedies

Bickel concludes his development of a morality of consent by applying it. Although his last chapters might be said to be about the rights of speech, civil dissent, conscience, and free inquiry, Bickel himself speaks differently and seeks to have us speak differently. His chapter on the First Amendment is entitled "Domesticated Civil Disobedience," for example, and the chapter treating of the universities is called "Moral Authority and the Intellectual." Between moral absolutes and moral relativity Bickel seeks a practical mean by his morality of the process, thus to indicate reasonable priorities among the variety of goods and lesser evils embraced by our legal order. Granting for now the general outlook that we have questioned before, these fresh and dialectical reasonings are probably the best examples of the political-constitutional wisdom that Bickel sought to provide.

Bickel starts with the First Amendment, given first priority by modern writers and judges. He accepts Justice Brennan's very modern understanding of the purport of its protection of speech and press: debate on public issues is to be "uninhibited, robust, and wide-open" (1975, p. 58). But how much so? Not so uninhibited as the familiar shout of "fire" in a crowded theater. Bickel will go along with the "clear and present danger test" in limiting speech that may cause a deed violating the law. But that is not sufficient. We sometimes permit speech even when lawbreaking is involved. Although publication of the *Pentagon Papers* violated the federal government's security classifications, the *Times* was permitted to publish them. In the interests of free speech and press, courts permit "domesticated civil disobedience." To what purpose? Bickel argues the political interest in such freedom to be paramount, especially the successful operation of the political process. There are involved "those activities of communication by which we govern" (1975, p. 67). Free speech is no "absolute," then. In the *Pentagon Papers* case, the government could not show a threat to national security, and the threat of "embarrassment" that it did reveal was an insufficient consideration. The question would have been stickier, however, if the publisher had been the purloiner. Also, interests other than free political communication may impinge. Criminal juries unbiased and uninfluenced by press accounts may be well worth limits on the press. Another and more controversial reason: to prevent "unacceptable results" of the political process (1975, pp. 67–70).

Bickel sharply attacks Holmes' doctrine that "the best test of truth is the power of the thought to get itself accepted in the competition of the market." Does market acceptance justify "segregation" or "genocide" or "proletarian dictatorship?" "Disastrously, unacceptably noxious doctrine can prevail" (1975, p. 72). The marketplace theory of truth, child of a faith in progressive historical consciousness, proves childishly trusting. Besides, even speech that does not advocate ideas has had noxious effects. The Supreme Court's laissez-faire policy toward slander, "assaultive" speech, and obscenity has tended to undermine the moral and civil preconditions of serious political debate, Bickel thought. For example, some speech is almost "verbal violence," itself "bullying." There is a difference between a sign, "Down with the Draft," and another sign, loosely translated as "Fornicate the Draft" (1975, pp. 72–73). The Court has not perceived it. Also, violent and perverse speech creates a nasty climate of action. "Where nothing is unspeakable, nothing is undoable" (1975, p. 73). Whatever the "right of privacy," in any event, should one permit those with a "taste for the obscene" to influence the environment of theater districts, of central cities, the tone of the neighborhood news shop and corner drug store? Should the Court provide for a "minimal definition of the good, true, and beautiful," for what Bickel (albeit weakly) calls "majority tastes?" Without "rules of civil discourse" even a marketplace turns into a "bullring" (1975, pp. 72–77).

Other accommodations are called for, not least in the relation between press and government. In such matters as the news reporter's protection of confidences, Bickel elaborates an adversary relationship between an inquiring press and a secretive government. He interprets the two as "countervailing powers," such that their rivalry serves the public interest. In a pinch he favors the press since "secrecy and the control of news" is all too tempting for governments, although he recognizes a need for exceptions (1975, pp. 79–88).

"Ambiguity and ambivalence" is the theory or at least condition of the First Amendment. We can now see why Bickel so insists. He begins with an absolute notion of liberty (uninhibited, robust, and wide-open), and then shows that the public safety, law-abidingness, the public interest, national security, justice, political and moral decency, serious and informative discourse—all limit the sort of public speech that a prudent and thinking individual would favor. That absolute beginning, however, was a foolish beginning. Bickel starts with the "bull-ring" or "marketplace" theories whose simplicity and foolish hopes he has exposed. These gone, we can return, not to this or that pat solution, but to the moderate liberal understanding of speech and free speech, the understanding that characterized earlier courts and such statesmen as Lincoln, Marshall, Jefferson, and others. While there will be room for debate enough, only moral and political naivete could call ours a legacy of suppression.

Bickel's treatment of conscientious objection to military service and civil disobedience of the law is particularly illuminating (1975, pp. 91–111). Conscientious objection has turned toward coercion and thus civil disobedience, he contends; civil disobedience has turned toward revolution. Bickel defends both conscientious objectors and the civilly disobedient, but in a way meant to preserve a free legal order. Bickel's way respects individual "values," although only for their basically "political" function. Civil disobedience, for example, would not be permitted in a "unitary order." In ours, however, it engenders a real consent, one born of "continual responsiveness" of government. Neither the ballot nor speech can so express "intensities of need and interest" (1975, p. 100). There can even be a "negative law formation" by protest: the toppling of President Johnson in 1968 (1975, p. 102). In general, a majority should reconsider a war opposed resolutely by a substantial minority on political or moral grounds, and also should reconsider some domestic policies pursued by the courts (including busing for racial balancing). There are problems, already noted, in Bickel's remedy: in his substitution of consent for governing and of the encouragement and limits of consent for the ends and limits of government. Here he raises the civilly disobedient along with the press to the status of legitimate lawbreakers and rival lawmakers.

Nevertheless, Bickel also sets forth a list of the minimal limits on civil disobedience needed by even so tolerant a democracy as ours and broken repeatedly in recent years. Even to these limits, of course, exceptions may be warranted, but rigid reverence for law and procedure is not now our chief problem. Bickel condemns disobedience to particular court decisions (as opposed to political struggle against the rule of the case); resort to violence; the leaking of official secrets instead of resigning one's post; the confusion of civil disobedience with revolution against the system; and, finally, the visible and consequent erosion of habits of law-abidingness (1975, pp. 112–17). "What is happening to morality today?" Bickel was asked at a panel. "It threatens to engulf us," he replied (1975, p. 119). The legal order has groaned and heaved under a "prodigality of moral causes," and under continuous assault it has been severely wounded. The worst wounds are from within–from the Supreme Court itself. The Court as lawbreaker in a deep sense, even as the supreme teacher of contempt for legal procedure and fundamental law in the name of what seemed "right" and "good"–that is Bickel's last word, certainly not his only word but nevertheless his final word, on the Warren Court (1975, pp. 120–21).

By the universities also wounds have been given, and yet Bickel portrays their recent travail as hurtful chiefly to themselves. An uncivil civil war was waged, an intense microcosm of the political siege without. This was not strange, since the war was one of ideas and ideology. The most violent and base rhetoric was aimed at the universities, both home of the old rationalism and breeder of the new antirational militants. This is Bickel's most moving chapter, because of his love for studies, because of his deep awareness of the indictment hurled at people like himself, and also because here he confronts the hollowness of the "process" he defends.

The struggle for "student power," he says, is inevitably a struggle over the very purpose of the university. The students seeking power tend to be "anti-intellectual, anti-professional," wishing to "feel" and "be" and to commit the universities to immediate political ends (1975, pp. 128–29). Bickel thinks this will make of colleges various enclaves of those variously committed, thus causing sectarianism and ending free inquiry; that before these moral imperatives standards will fall and the deserving will be slighted and neglected; that balance and reason will be sacrificed to irrational commitment (1975, pp. 130–36).

Against this challenge Bickel expounds "freedom of inquiry" as the university's proper purpose. Such inquiry is "neutral" and "agnostic" on political matters and not committed to any political or ideological position. The university is not "the practical servant of the society"; intellectuals may be committed to thought without action, or thought that may "oppose action" (1975, pp. 128–29).

There is a severe difficulty here. What distinguishes Bickel's goal from that of those "empty rationalists," irrelevant and cowardly in their inability to judge whether any important change is better or worse? Indeed, many students find historicist historians of "ideas," positivist scientists of society, to be deadly boring not merely in their politics but also in their specialties. Why should this "method of reason" be thought alive enough to be worth pursuing?

Bickel's own lively yet serious inquiry indicates, however, what his own procedural account of inquiry does not. He inquires seriously about the dangers and purposes and arrangements involved in our law and politics and in good law and politics. Thus he can enjoy and teach others to enjoy a wisdom that can cultivate practical wisdom, because it is more than methodological or historical or neutral. His inquiry is reminiscent of jurisprudence and political science of an ancient kind, of a kind that was liberal like liberal education in liberating from zealotry without liberating from justice. Still, one must say more. Even university studies in the best sense may be subversive, apart from historicist apathy and methodological relativity and the committed's revolutionizing. Serious inquiries question premises, even the premises of our law and politics. Yet, as Bickel perceives, one's devotion to the university of free inquiry implies a certain loyalty to the order which sustains such a university. It implies a "generalized allegiance" to the legal process (1975, pp. 129–30). It requires more. Intellectuals in particular should stand by "the fundamental principles and attitudes of liberalism, . . . the essence of the American political tradition" that touches "Lincoln's mystic chords of memory" (1975, p. 139).

Much can be learned about this country's law and politics, and even about law and politics in general, by considering the deeds and speeches of such a political tradition. A more widespread teaching of the American tradition of constitutional and political thought would help students and teachers alike to avoid mindless subversion, while also avoiding mindless loyalty. One could start with a few of Lincoln's speeches; no American statesman, perhaps no statesman, better combined humanitarian seriousness with political prudence. Perhaps one should make a point these days of attending to our constitutional law and constitutional commentaries, notably formative ones like *The Federalist.* In his three major works Bickel sought to show how politic judgment should guide those who interpret our Constitution. These works proceeded in a kind of circle. It is also necessary to show to both judges and political leaders the wisdom of abiding by the Constitution.

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A Reactive Linkage Model of the U.S. Defense Expenditure Policymaking Process*

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Several alternative explanations of U.S. defense expenditure policy-making behavior have been heralded as theoretically sound and empirically supported representations of U.S. defense expenditure policy making. This has created an atmosphere in which almost as much effort is directed towards criticizing and/or defending existing explanations as is given to improving them through continued theoretical and empirical investigations. To overcome this impasse, I have developed in this article a working synthesis of three current approaches—Arms Race, Organizational Politics, Bureaucratic Politics—*based on the insights and observations of Samuel Huntington (1961), Warner Schilling (1962), Colin Gray (1971), and James Rosenau (1971). The resulting formulation, which is labeled the* Reactive Linkage Model, *characterizes the pattern of U.S. defense spending in terms of an initial response by the armed services to the anticipated USSR expenditure level which is subsequently filtered through the president, Congress, and the Department of Defense to determine the magnitude, scale, and timing of the reaction.*

On the basis of the theoretical and empirical analyses here presented, I have reached the following conclusions. First, the Reactive Linkage Model successfully synthesizes the three existing explanations of U.S. defense expenditure policy making into a coherent and plausible whole. Second, the estimated version of the model provides an accurate representation of the pattern of U.S. defense expenditure policy making from 1954 to 1973. Specifically, the individual equations account for a large portion of the variance with parameter estimates that are both sharp and plausible and the model as a whole generates extremely accurate historical forecasts. Finally, I investigate the impact of war, presidential party, and negative public opinion on individual decisions, basing my inquiry on the assumption that the model accurately represents U.S. defense expenditure policy making. The results show that different events have an impact on only some of the steps in the policy-making process and the steps which are so affected vary from event to event. As a consequence of these results, I argue that the Reactive Linkage Model, and hence the idea of combining the key elements from competing explanations, represents a positive step forward in the study of U.S. defense expenditure policy making.

Over the past several years various explanations of U.S. defense expenditure policy making have been presented which stipulate both the environments in which the policy makers operate and the nature of the defense policymaking process (e.g., Huntington, 1961; Schilling, 1962; Russett, 1970; Stromberg, 1970; Crecine, 1971; Kanter, 1972; Strauss, 1972; Crecine and Fischer, 1973; Korb, 1973; Gist, 1974; Ostrom, 1977). These explanations can be classified into three distinct categories of explanations, (1) *Arms Race*, (2) *Organizational Politics*, and (3) *Bureaucratic Politics*, which differ in their respective characterization of the principal environmental stimuli as (1) primarily international, (2) primarily domestic, and (3) a combination of the two. Meanwhile we may describe the U.S. defense expenditure policymaking process as, respectively, (1) unitary/rational, (2) organizational, and (3) bureaucratic. Because of these differences, students of U.S. defense expenditure policy making are confronted with a number of somewhat complementary, somewhat contradictory explanations. This, in turn, has created a research atmosphere in which almost as much effort is directed towards criticizing and/or defending existing explanations as towards continuing theoretical and empirical investigations.

It appears, then, that research on U.S. defense expenditure policy making has reached an impasse which must be circumvented. One possible strategy is to compare and evaluate the contending models. However, where this strategy has been implemented (e.g., Ostrom, 1977), none of the models has performed well empirically.[1] An alternative strategy, suggested by

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[1] While all of the previous work on budgeting reports very high R^2s, these results can be quite

Allison (1971, p. 257), is to determine the complementary and conflicting aspects of the existing explanations followed by the formalization of a "synthetic" model which builds upon the similarities while attempting to resolve the inconsistencies. It is this strategy which I will pursue in this article.

A justification for a synthesis of the existing approaches lies in the fact that the three models are complementary in terms of their environmental stimuli. Several authors usually associated with *Bureaucratic Politics* (e.g., Huntington, 1961; Schilling, 1962; Gray, 1971) have suggested linking the *Arms Race* and *Organizational Politics* explanations by asserting that single-factor explanations are incomplete. Their general orientation is captured in James Rosenau's linkage concept (1971, p. 305) which implies that one "cannot study the internal behavior of nation-states independently of the larger context in which it occurs and toward which it is directed." That is, although final foreign policy decisions are usually made by single nation-states, these states exist in, are conditioned by, and respond to the international environment.[2] Thus a complete understanding of foreign policy behavior can be gained only through a simultaneous consideration of national and international environments *and* the linkages that exist between them.[3]

An impediment to the proposed synthesis is that the three models differ widely in their conceptualization of the defense policy-making process. The *Arms Race* and *Bureaucratic Politics* models are polar opposites; the former views the process as if it were the decision of a single individual (e.g., Allison, 1971, p. 32) while the latter views the process in terms of all individuals involved in the decision (e.g., Allison, 1971, pp. 162–64). Between these two views is the *Organizational Politics* explanation which conceptualizes the policy-making process as if it were "a conglomerate of semi-feudal, loosely allied organizations, each with a substantial life of its own," and sees the resulting decisions "less as deliberate choices and more as the output of large organizations" (Allison, 1971, p. 67). Because of the apparent incompatibility of the three conceptualizations, I will make no attempt to integrate them into a single model of the defense policy-making process; rather, I will view it in organizational terms. Consequently, the synthetic model which follows is based on behavioral organization theory (e.g., Simon, 1945; March and Simon, 1958; Lindblom, 1959; Cyert and March, 1963; Crecine, 1969) which is plausible in the defense context (e.g., Crecine and Fischer, 1973, p. 209), has met with success in modeling other forms of decision-making behavior (e.g., Davis, Dempster, and Wildavsky, 1966a; Crecine, 1969; Newell, 1972; Hoole, 1976), and represents a compromise, in terms of complexity, between the conceptualizations offered by the *Arms Race* and *Bureaucratic Politics* models.

Having determined that the environmental stimuli are complementary and having chosen the organizational policy-making perspective, I can advance the design outline of the synthetic model. Following the previously articulated *Organizational Politics* perspective (Ostrom, 1977), I will factor the U.S. defense expenditure decision into four temporally ordered steps which represent the actions of quasi-independent organizations (i.e., armed services, president, Congress, Department of Defense). Each organization will be viewed as if it possesses a single, stable decision rule based upon a small number of indicators, simple rules of thumb, and a historical base. Formally, the policy-making rules will be specified as:

$$D_{it} = b_{i1}H_{it} + \sum_{i=1}^{n} \sum_{j=2}^{k} b_{ij}E_{jt} + u_{it} \qquad (1)$$

where D_{it} is the ith organization's decision, H_{it} is i's historical base, E_{jt} are the exogenous factors (i.e., environmental stimuli), u_{it} are random disturbance terms, and b_{ij} are the rules

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misleading. First, as Wanat (1973) suggests, due to the constant dominance of budgetary data from successive steps one is likely to find high R^2s. Second, by introducing baseline considerations into the evaluation, Ostrom (1977) shows that existing models are not particularly accurate forecasting instruments.

[2] The logic of such a fusion has been cogently presented by Gray (1971, p. 77): "If our understanding of the arms race phenomenon is to improve, the need is for synthesis.... The arms race participants must maintain a general attention to the developing military posture of their rivals.... However, the domestic processes of a state will determine whether there are to be reactions or not, ... and also the scale and timing of a reaction."

[3] As examples of this orientation, consider the following two arguments: "The kinds of defense a budget provides will be primarily a reflection of the kinds of ideas people have about the political-military world in which they are living.... But the influence exercised on the content of the budget by the character of the political process, while definitely subordinate, is not insignificant" (Schilling, 1962, p. 15). "... The competition between the external goals of the government as a collective entity in a world of other governments and the domestic goals of the government and other groups in society is the heart of military policy" (Huntington, 1961, p. 3).

of thumb or policy parameters.[4] The actual operationalization of the elements in Equation 1 specifies the organization making the decision, its historical base for the decision, and the specific domestic and/or international factors affecting the decision. The incorporation of organizational, domestic, and international factors into the individual policy-making rules reflects the synthesis of the *Arms Race*, *Organizational Politics*, and *Bureaucratic Politics* models.

Reactive Linkage Model

Of the various types of linkages detailed by Rosenau (1971, p. 320), the reactive linkage seems most promising in explaining the U.S. defense expenditure policy-making process. The adoption of a reactive linkage orientation suggests a way in which the domestic and international environments can be combined with the organizational decision-making process. The initial policy-making rule (i.e., services' request for funds) can be viewed as a reaction to the changing conditions in the international and domestic environments which is then filtered through the remaining organizations (i.e., president, Congress, Department of Defense) to determine the magnitude, scale, and timing of the reaction. The schematic of the *Reactive Linkage* explanation is presented in Figure 1.

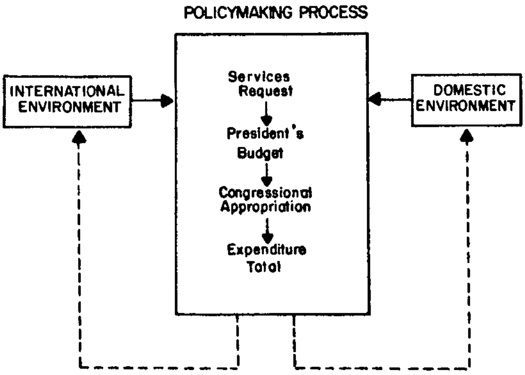

Figure 1

Schematic Drawing of the Linkage Model

Assuming policy-making rules of the form specified in Equation 1, the four organizational actors react to their historical bases as well as to the domestic and international environments. The following equations constitute the *Reactive Linkage Model* of the U.S. defense expenditure policy-making process:

$$y_{1t} = b_{11}x_{1t-1} + b_{12}x_{2t-1} + b_{13}x_{3t-1} + u_{1t} \qquad (2)$$

$$y_{2t} = b_{21}y_{1t} + b_{22}x_{4t} + u_{2t} \qquad (3)$$

$$y_{3t} = b_{31}y_{2t} + b_{32}x_{5t} + u_{3t} \qquad (4)$$

$$y_{4t} = y_{3t} + x_{6t} \qquad (5)$$

$$y_{5t} = b_{51}y_{4t} + b_{52}y_{5t-1} + u_{5t}, \qquad (6)$$

where b_{ij} are policy parameters, u_{it} are random disturbance terms, y_{it} are endogenous variables, and x_{jt} are exogenous variables. The definitions for y_{it} and x_{jt} are presented in Table 1. In terms of the components of Equation 1, the y_{it} on the left-hand side of the equations represent D_{it}, the y_{it} on the right-hand side of the equations represent H_{it}, and the x_{jt} represent E_{jt}. In order to identify the assumptions underlying each equation, I will discuss in detail individual policy-making rules.

Services' Request. As postulated, the services' request establishes a reactive linkage with the environment by way of the following policy-making rule:

$$y_{1t} = b_{11}x^*_{1t} + b_{12}x_{2t-1} + b_{13}x_{3t-1} + u_{1t} \qquad (7)$$

where the asterisk denotes the anticipated level of USSR defense spending. Thus, as presented in Equation 7, the U.S. defense expenditure policy-making process is initiated by a reaction to three indicators of defense need. The first priority of the services is to maintain a level of defense spending that is sufficient to deter/contain the USSR which poses the "gravest threat to the national security of the United States" (NSC–68, 1975, p. 103). Because of this threat and because of the problems associated with immediately increasing defense expenditures, it is necessary for the U.S. to keep a

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[4] Two special characteristics must be noted. First, each of the policy-making rules explicitly excludes a constant term. The reason for the exclusion is substantive and reflects Davis, Dempster, and Wildavsky's observation (1966a, p. 532) that policy makers think in percentage terms. Second, each of the policy-making rules contains a random disturbance term which represents the exigencies of the domestic, international, and policy-making environments. Although it is assumed at each step that the decision is determined by the policy parameters and the variables, these factors do not account for all of the variation in the decision. Assuming that the omitted factors are not systematic, it is possible to appeal to probability theory and assume the total effect of all variables not appearing in the equation can be approximated by a random disturbance term.

Table 1. Definitions, Operationalizations, and Data Sources

Variable	Definition	Operationalization	Source
Services Request (y_{1t})	Proposal by armed services for amount of defense expenditure each fiscal year	Joint Chiefs of Staff October 1 submission to the Secretary of Defense	Office of the Assistant Secretary of Defense (Comptroller), Director for Program and Financial Control
President's Budget (y_{2t})	Presidential defense budget request to Congress each fiscal year	Budget proposed by the Office of Management and Budget for "Department of Defense–Military"	Congressional Quarterly, *Almanac* (92nd Congress, 2nd Session, Vol. 28, p. 802)
Congressional Appropriation (y_{3t})	Amount of money appropriated for defense each fiscal year by Congress	Congressional appropriation for "Department of Defense–Military"	Congressional Quarterly, *Almanac* (92nd Congress, 2nd Session, Vol. 28, p. 802)
Total New Obligational Authority (y_{4t})	Total amount of new defense appropriations each fiscal year	Total new obligational authority (appropriations plus supplementals) available to "Department of Defense–Military"	*The Budget of the United States Government* (Washington, D.C.: 1954–1973)
Expenditures (y_{5t})	Total defense expenditures by the U.S. each fiscal year	"Department of Defense–Military" expenditures	*The Budget of the United States Government* (Washington, D.C.: 1954–1973)
USSR Expenditures (x_{1t})	Total defense expenditures by the USSR each fiscal year	Military expenditures by USSR Ministry of Defense	International Institute of Strategic Studies, *Military Balance*
U.S. Battle Deaths (x_{2t-1})	Number of U.S. servicemen killed in wars	U.S. casualties which result from enemy action	*Statistical Abstract of the United States*
Trend in Congressional Appropriation (x_{3t-1})	Rate at which congressional appropriations have been increasing	$y_{3t-1} - y_{3t-2}$	

Table 1. Definitions, Operationalizations, and Data Sources (continued)

Variable	Definition	Operationalization	Source
Ratio Goal (x_{4t})	Ratio between desired and actual U.S. defense expenditure levels	$1.275x_{1t} - y_{5t-1}$	
Congressional Mood (x_{5t})	Factor representing the presence of cutting and spending moods	$y_{3t-1} - .968y_{2t-1}$	
Supplemental Appropriation (x_{6t})	Additional appropriations for defense which are the result of special circumstances	Supplemental appropriations for "Department of Defense–Military"	Congressional Quarterly, *Almanac*
War (z_{1t})	Whether or not the U.S. is involved in a war	0 if there are less than 1000 casualties in year *t* 1 if there are more than 1000 casualties in year *t*	*Statistical Abstract of the United States*
President's Party (z_{2t})	Whether or not the president is a Democrat	0 if the president is a Republican 1 if the president is a Democrat	
Public Opinion (z_{3t})	Whether or not there is a high proportion of negative public opinion against increased defense spending	0 if the public opinion does not favor decreased spending 1 if public opinion does favor decreased spending	*Gallup Opinion Index*, April 1976 Report No. 129

ratio of clear superiority (i.e., $b_{11} > 1$) to the anticipated level of USSR defense spending even in times of relative peace.[5] By doing so, the U.S. will "develop a level of military readiness which can be maintained as long as necessary as a deterrent to Soviet aggression . . . and as an adequate basis for immediate military commitments and for rapid mobilization should war prove unavoidable" (NSC–68, 1975, p. 106).

Having requested an amount sufficient to deter/contain the USSR, the armed services add an amount based on additional defense requirements. During the post-World War II era the prime sources of such additional requirements have been the Korean and Vietnam wars. If, as is being assumed, such increases are proportional to the magnitude of the war and if the number of battle deaths is a plausible indicator of magnitude, the services are likely to request money in proportion to the number of battle deaths during the previous year (i.e., $b_{12} > 0$).[6]

The final component of the services' request is a function of the strategy of agencies in the budgetary context (Wildavsky, 1964). If the services' request were excessive, it is possible that Congress would retaliate in subsequent years by reducing the defense portion of the federal budget. As a consequence, the services try to anticipate the behavior of Congress. One plausible indicator of the congressional orientation toward the service request is the trend in congressional defense appropriations (Crecine and Fischer, 1973, p. 211). If Congress has been increasing appropriations to defense, the services will increase their request to take advantage of this tendency; however, if Congress has been consistently decreasing appropriation levels, the services will lower their request (i.e., $b_{13} > 0$).

To operationalize Equation 7 it is necessary to introduce an assumption concerning the services' formulation of their expectation of USSR defense expenditures. In the context of this model I have chosen to employ the adaptive expectations assumption (Kmenta, 1971, p. 474). Specifically, it is assumed that the services devise an expectation based on a weighted average of past USSR behavior; that is,

$$x^*_{1t} = (1 - \delta)\, x_{1t-1} + \delta x_{1t-2} + \delta^2 x_{1t-3} + \ldots]. \tag{8}$$

Based on both theoretical and empirical considerations, I assume that $\delta = 0$,[7] which reduces equation 8 to the simplest possible adaptive expectations assumption; that is,

$$x^*_{1t} = x_{1t-1}. \tag{9}$$

Thus, the USSR defense expenditure level is expected to take on the same value it held during the previous year. When x_{1t-1} is substituted for x^*_{1t} in Equation 7, the services' policy-making rule is identical to Equation 2.

Therefore, the services, through an adaptive expectations process, formulate a request based upon a reaction to the international and domestic environments. Each of these reactions is "strategic" with the desired result being both to provide for the common defense and to maintain the defense share of total U.S. federal expenditures. Given the formal services' request, the remaining steps of the process determine the magnitude, scale, and timing of the reaction.

President's Budget. Upon completion, the services' request is transmitted to the president. It is assumed that the president balances two conflicting roles in making a decision. First, a president must consider the defense budget in the context of the entire U.S. federal budget (e.g., Huntington, 1961, p. 218). This implies a view of the services' request as a stable, though padded, index of U.S. defense needs. The president's first reaction, then, is to cut the request based upon the assumption that the U.S. can deter/contain the USSR with less money; hence b_{21} is expected to be less than 1.00. In this role, the president takes the entire U.S. fiscal picture into account and apportions the defense sector its share. To be sure, there are occasions when only very small cuts are made in the services' request (e.g., 1959, 1961). These occasions are not random but are related to the president's second role, that of commander-in-chief. This orientation comes through in President Gerald R. Ford's justification of his 1977 defense budget:

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[5] To be sure, the services are concerned with much more than the total USSR defense expenditure level. Indeed, there is a wide range of inputs into the services' policy-making calculus. The USSR expenditure total is used because of its symbolic preeminence; that is, it is one measure that is widely accepted as an indicator of Soviet strength, is easily quantifiable and understood, and can easily be compared to U.S. spending levels. The use of the simple indicator is justified furthermore by the organizational perspective used in this paper. From such a perspective the policy makers simplify the decision making by using simple and easily understood indicators.

[6] Given the incremental nature of the U.S. defense budgetary process, these war-related increases are likely to become part of the base in subsequent years. The inclusion of such a factor in the services' calculus may help to account for what Russett (1971, p. 2) calls the "ratchet effect" of wars on defense spending.

[7] In order to justify the use of this simplified version of the adaptive expectations model, consider the following. First, its simplicity is consistent with the organizational policy-making perspective. Second, the stipulation that $\delta=0$ is supported by the data. That is, when Equation 8 is substituted into Equation 7, a Koyck lag is employed (Kmenta, 1971, p. 475), and the resulting equation is estimated, the estimate of δ is small and insignificant. Note that the assumption of $\delta=0$ means that the services' request does not have a historical base in the usual sense. In place of y_{1t-1} (which would be present in Equation 2 if $\delta \neq 0$), it seems reasonable to view x_{1t-1} as the historical base.

> "I am recommending a significant increase in defense spending for 1977.... My request is based on a careful assessment of the international situation and the contingencies we must be prepared to meet.... We dare not do less" (Quoted in the *New York Times*, January 26, 1976).

Thus even though a president is interested in reducing the size of the services' request, he is also sensitive to the international balance of power.

As I have chosen to characterize the situation, the president attempts to realize a ratio goal in the context of his second role. Basing my conclusion on Caspary's (1967) reinterpretation of Richardson (1960), I have defined a ratio goal (x_{4t}) as

$$\beta_1 x_{1t} - y_{5t-1}. \tag{10}$$

Over time, presidents desire a balance of power which maintains an equality between desired U.S. spending (i.e., $\beta_1 x_{1t}$) and the level at which the U.S. has been spending (i.e., y_{5t-1}). Reacting positively to this ratio goal (i.e., $b_{22} > 0$), the president will increase a budget request whenever $\beta_1 x_{1t} > y_{5t-1}$ and decrease a budget request whenever $\beta_1 x_{1t} < y_{5t-1}$. Of the possible strategies concerning the operationalization of x_{4t}, I have chosen to assume that the president has maintained the ratio goal at zero over time.[8] For this to be the case

$$E[x_{4t}] = E[\beta_1 x_{1t} - y_{5t-1}] = 0 \tag{11}$$

so that

$$E[\beta_1] = \bar{y}_{5t-1}/\bar{x}_{1t} \tag{12}$$

where $\bar{y}_{5t-1}$ and $\bar{x}_{1t}$ are the variable means. Finally, because of responsibilities in both the defense and domestic areas, the president is able to add only a fixed amount of the ratio goal to his request; that is, $0 < b_{22} < 1$. This means that the president is only able to maintain the desired balance of power in an incremental manner.[9]

Congressional Appropriation. In most budgeting studies, Congress is cast in a role as guardian of the public purse (e.g., Davis, Dempster, and Wildavsky, 1966a). While there is no doubt that Congress has an impact on the defense budget, there is some question whether it has a fiscal or a programmatic orientation toward defense considerations. Kanter (1972) and Korb (1973) disagree over the validity of the common assumption that Congress adopts a fiscal role. Kanter (1972, p. 130) observes that support for the fiscal hypothesis is based on a "superficial reading of highly aggregated data" while Korb (1973) concludes that "congressional motivations are ... mostly fiscal." Kanter's point is worthwhile and needs to be investigated using different levels of aggregation of the defense budget.[10] However, following Korb's argument and convincing empirical evidence, I have chosen to incorporate the fiscal hypothesis into the present model. Under the fiscal hypothesis Congress seeks to reduce the overall level of defense spending by employing the following type of across-the-board percentage cuts:

$$y_{3t} = b_{31} y_{2t} + v_t \tag{13}$$

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[8] There are at least two other ways to approach the estimation of this equation. First, one can regress y_{2t} on y_{1t}, x_{1t}, and y_{5t-1} (i.e., disaggregate x_{4t}). The problem with this approach is that there is an extreme multicollinearity problem since x_{1t} and y_{5t-1} are correlated at .96. By simply going ahead in face of this problem we find the parameter estimates are very unstable and lose all semblance of interpretability. Second, one can use a constrained estimation method, that is, use a technique which allows one to impose a certain range of values on the parameters (e.g., $.80 < b_{21} < 1.00$). The problem with this approach is that the estimation packages which I have available do not allow such inequality constraints. Furthermore, the imposition of such constraints requires the same type of prior information used in specifying $\beta_1 = \bar{y}_{5t-1}/\bar{x}_{1t}$.

[9] In Richardson's terminology, $1/b_{22}$ can be viewed as the apparent catch-up time so that if the USSR were to maintain the same spending level over time (and if the president's budget is appropriated and spent), the ratio goal will be realized in $1/b_{22}$ years.

[10] In an unpublished manuscript (Ostrom, 1976), I have replicated Davis, Dempster, and Wildavsky's analysis (1966a) for the 33 line items in the defense budget used by Kanter (1973). Contrary to Kanter's assertions, the results of this preliminary analysis support the fiscal hypothesis.

where b_{31} is expected to be less than 1.00 because of Congress' assumed fiscal role.

The deviation, v_t, results from special circumstances (e.g., end of a war, negative public opinion, unemployment, international threats) which lead Congress either to make more substantial cuts than usual or to appropriate more than has been requested. Furthermore, the following two examples are representative of the evidence that such deviations are systematic over time and result from cutting and spending moods (Wildavsky, 1964). First, a recent study of the U.S.-USSR military balance by Collins and Chwat (1976, p. 16) acknowledges the presence of "cyclical cutbacks" in defense spending by Congress in the aftermath of every war. Second, John W. Finney (1976) notes the presence of a spending mood in the following observation:

> Even before the Congressional hearings are completed the Ford administration appears to be winning the annual debate with the argument that the United States must counter a growth in Soviet military power.... *Out of a series of interviews emerged a marked change in attitude ... about the desirability of challenging the basic defense budget submitted by the administration* (Emphasis added).

Thus in the 1970s, Congress has exhibited both cutting and spending moods. It is reasonable, therefore, to expect special circumstances to arise from time to time which result in Congress' momentarily departing from its usual policy-making rule (i.e., Equation 13). Whatever the reasons for such deviations, they are of short duration and Congress gradually returns to its normal behavior.

While such moods have not been adequately studied or operationalized (Wildavsky, 1964, p. 72), they can be represented by a stochastic disturbance term (Davis, Dempster, and Wildavsky, 1966a, p. 535). Specifically, the behavior of cutting and spending moods can be modeled by assuming that the disturbance term in Equation 13 follows a first-order Markov scheme:

$$v_t = b_{32}v_{t-1} + u_{3t} \qquad (14)$$
$$0 < b_{32} < 1.$$

Since b_{32} is constrained between 0.00 and 1.00, once a deviation occurs Congress tends to decrease subsequent deviations steadily back toward 0.00 as long as there are no systematic factors represented by u_{3t}. Substituting Equation 14 into Equation 13 yields Equation 4.[11] This form of the congressional policy-making rule acknowledges the fact that even though cutting and spending moods do have an impact on congressional defense appropriations, not much is known about the generation of moods.

Supplemental Appropriation. Unlike the other steps in the U.S. defense expenditure policy-making process, that which includes supplemental appropriations has not been extensively studied. In fact, most students of the budgetary process have explicitly excluded supplementals from their analyses because "the factors affecting supplemental appropriations are statistically independent of those influencing regular appropriations" (Bowman, et al. 1967, p. 99). I have included supplementals in this study because they constitute a large percentage of the defense appropriation in certain years and because they are part of the Department of Defense's yearly appropriation. However, because of previous findings, I have specified the relationship as an identity—total new obligational authority is merely the sum of the regular congressional appropriation and the supplemental appropriation. Strictly speaking, this is not a policy-making rule.

Expenditure. The final step in the U.S. defense expenditure policy-making process concerns implementation. There is a potential hazard in specifying the expenditure policy-making rule since the Department of Defense has massive obligated and unobligated reserves most of which could conceivably be spent in a given fiscal year. I argue that this does not pose a problem since the Department of Defense makes its decision in the face of a number of constraints on its behavior. First, it is dependent on Congress for future appropriations and hence must appear sensitive to congressional desires by spending approximately what is appropriated. That is, the base for the Department of Defense decision is the total amount of newly appropriated money each fiscal year. Second, due to institutional constraints, public opinion, and bureaucratic inertia, the Department of Defense is able to realize only a portion of its desired spending level.

In formulating the expenditure policy-making rule, I assume that the Department of Defense formulates a desired level of spending,

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[11] The functional form of the congressional policy-making rule is taken directly from Davis, Dempster, and Wildavsky (1966a). The substantive interpretation presented in this article is somewhat different from theirs.

y^*_{5t} which is based on the total new obligational authority, y_{4t}:

$$y^*_{5t} = \beta_2 y_{4t} + e_{1t} \qquad (15)$$

where β_2 is the percentage the department wishes to spend and e_{1t} is a random disturbance term. Due to the second set of constraints mentioned in the previous paragraph, I have incorporated into the model a partial adjustment assumption (Kmenta, 1971):

$$y_{5t} - y_{5t-1} = \lambda(y^*_{5t} - y_{5t-1}) + e_{2t} \qquad (16)$$
$$0 < \lambda < 1.$$

That is, the actual increase in expenditures from the previous year, $y_{5t} - y_{5t-1}$, is only a percentage of the perceived optimal increase, $y^*_{5t} - y_{5t-1}$. Solving Equation 16 for y^*_{5t} and substituting the result into Equation 15 yields the expenditure policy-making rule presented in Equation 6. Note that in terms of the previous development $b_{51} = \beta_2\lambda$, $b_{52} = (1 - \lambda)$, and $u_{5t} = (\lambda e_{1t} + e_{2t})$. Thus Equation 6 provides an operational version of the partial adjustment hypothesis which underscores the fact that the Department of Defense is constrained in its attempts to implement its desired spending level. Not only is it tied to the total new obligational authority each year, but also due to a number of constraints it can realize only a portion of its desired spending level. In short, the Department of Defense is not free to set any expenditure total; it is an organization making a decision under constraints.

The Reactive Linkage Explanation. In combining the key elements of the *Arms Race* and *Organizational Politics* explanations, the Reactive Linkage Model succeeds in operationalizing and formalizing the suggestions of Huntington (1961), Schilling (1962), Gray (1971), and Rosenau (1971). The services' request captures the idea of a reactive linkage through the incorporation of a simplified adaptive expectations assumption. The president's budget represents the tendency of the president to cut the services' request while incrementally maintaining a ratio goal. The congressional appropriation is characterized as a fiscally motivated reaction to the president's budget which makes allowances for the presence of cutting and spending moods. Finally, the expenditure decision is viewed as one made by the Department of Defense under a number of constraints so that the final decision is only a partial realization of the desired spending level. On the basis of the discussion to this point I would argue that the Reactive Linkage Model synthesizes the three existing explanations of U.S. defense expenditure policy making into a coherent and plausible whole.

Before empirically evaluating the Reactive Linkage Model, we must acknowledge several limitations of the present formulation. First, the model does not resolve all of the conflicts among the three contending explanations; instead, it opts for trying the simplest synthetic model first. Second, it assumes that organizational actors continue to use the same policy-making rules over time. (Later in the paper I will investigate the impact of certain types of discrete shocks to see if they might temporarily alter the policy-making rules.) Finally, the model makes rather simple assumptions about the policy-making behavior of the USSR; specifically, it assumes that the USSR spends approximately what it has been spending in the past. This may be overly simplistic and hence further investigations of the U.S. defense expenditure policy-making process might incorporate more sophisticated assumptions about USSR defense expenditure behavior.

Empirical Evaluation

The definitions, operationalizations, and sources of the variables employed in the model are presented in Table 1. Data for these variables have been collected for the period 1954 to 1976. The level of aggregation is the expenditure total and each variable is measured in current dollars; while these choices are somewhat controversial, they have been discussed at length in a previous article (Ostrom, 1977). A second data-related consideration concerns the selection of Soviet data. This study uses the International Institute for Strategic Studies data set since it is the only one which includes Soviet budget items comparable to the "Department of Defense–Military" line of the U.S. budget, in current dollars, for the 1954–76 period.[12]

[12] As many have noted, there are tremendous problems with the USSR defense expenditure data. In this regard I would like to note the following observation: "The Soviet defense budget is secret. Segments are concealed under different civil headings. Expenditures are enumerated differently than in the United States. Rubles are difficult to convert accurately into dollars. Most Western calculations are therefore based on one or two methodologies. The first which manipulates published Soviet data to correspond with U.S. categories and exchanges rubles for U.S. currency, risks underestimating the Kremlin's expenditures. The second, which judges how much it would cost to duplicate visible Soviet defense efforts in U.S. dollars risks overestimation" (Collins and Chwat,

Since the Reactive Linkage Model is recursive and specifically excludes constants from each equation, a restricted least squares technique (Kmenta, 1971, pp. 431–32) is applied sequentially to each equation to obtain parameter estimates.[13] The resulting estimates and statistics are displayed in Table 2. Each of the equations explains over 91 percent of the total corrected variance and is highly significant at the .01 level. Before making substantive inferences based on the parameters of the estimated model, we must make sure that there are no problems with the underlying statistical assumptions.

In this regard, two common problems—autocorrelation and multicollinearity—must be investigated. First, given the time-series nature of the model, it is possible that autocorrelated error terms may play havoc with the substantive inferences (Hibbs, 1974). However, looking at the relevant test statistics, Durbin-Watson's d and Durbin's h (Johnston, 1972, pp. 249–54, 312–13), we see no indication of significant autocorrelation in the estimated residuals. A second potential problem results from the presence of multicollinearity in the expenditure equation. Multicollinearity may be a problem since y_{5t-1} and y_{4t} are correlated at .91. This is further substantiated by the presence of relatively large standard errors (in comparison to b_{11}, b_{21}, and b_{31}) for the estimates of b_{51} and b_{52}. Despite these indications, the two estimated coefficients are significantly different from zero at the .01 level. However, even though the problem is not too severe, we must keep in mind that these estimates may not be very precise (Kmenta, 1971, pp. 389–91). The above considerations do not overcome all of the possible methodological problems. Still, one can place some faith in the estimates and the resulting substantive inferences, having examined several of the assumptions underlying the estimation technique and thus ruling out some possibly confounding factors.

Since it appears that the estimated coefficients are correctly estimated and that the underlying statistical assumptions do not pose any insurmountable inferential problems, the estimated Reactive Linkage Model can be evaluated from a substantive point of view. The previous section comments on the expected value (i.e., sign and magnitude) of the estimated parameters in the context of discussing each of the policy-making rules. Table 3 presents the hypotheses implicit in these expectations and the test results. As can be seen, all of the hypotheses, with two exceptions, are supported at the .01 level. Since the two exceptions are supported at the .05 level, the estimated model appears to be quite consistent with the previous substantive discussion.

There is considerable evidence, therefore, that the Reactive Linkage Model is supported by the data. Not only are the underlying statistical assumptions satisfied but also there is strong empirical support for a wide range of substantive hypotheses. The services do react to a range of domestic and international stimuli. The president does cut the services' request while simultaneously trying to maintain a ratio goal. Congress cuts the president's budget but is significantly affected, from time to time, by cutting and spending moods. The Department of Defense is able to obtain only a portion of its desired increase each year.[14] On the basis of the equation-by-equation analysis, the estimated Reactive Linkage Model provides an excellent fit to each of the four decisions with

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1976, p. 17). The IISS data used in this study are likely to be underestimates. However, as long as the underestimation is consistent over time this should not cause any severe problems.

[13] There are a number of additional considerations which must be taken into account when estimating the four equations. First, given the discussion of the previous section, we can estimate the services and presidential equations with restricted least squares [RLS]. Second, the congressional equation estimates require a two-stage procedure. Equation 13 must be estimated with RLS to provide an estimate of b_{31} which can then be used in the computations of x_{5t}. The computed x_{5t} can then be used in conjunction with RLS to estimate Equation 3. The latter procedure may present estimation problems because of the presence of x_{5t}. Davis, Dempster, and Wildavsky (1966b, pp. 98–100) note that problems arise only if u_{3t} is autocorrelated. As can be seen in Table 2, this is not the case. Finally, the expenditure equation can be estimated directly with RLS (Kmenta, 1971, p. 487). Provided that there is no serial correlation, the parameter estimates will be consistent. As can be seen from Table 2 there is no evidence of significant serial correlation.

[14] A final issue concerns the plausibility of the estimates for the parameters presented in Equations 15 and 16. Based on the estimated expenditure equation, $\lambda = .61$; that is, the Department of Defense obtains, on average, only 61 percent of its desired increase each fiscal year. In addition, the value of β_2 is determined to be 1.05; thus the desired level of spending is 105 percent of the total new obligational authority each fiscal year. Therefore, due to the previously elucidated constraints, the Department of Defense spends no more than 5 percent more than it is appropriated. Not only do these values cast light on the substantive interpretation of the Reactive Linkage Model but each seems very plausible in the U.S. defense expenditure context.

Table 2. Coefficient Estimates for the Reactive Linkage Model

Equation	Period[e]	b_{i1} (Standard Error) (t-ratio)	b_{i2} (Standard Error) (t-ratio)	b_{i3} (Standard Error) (t-ratio)	$\bar{R}^2$	Standard Error of Estimate	Auto-Correlation Test
Services Request	1955–73	1.421 (.042) (33.13)[a]	2.092 (.418) (5.00)[a]	.677 (.300) (2.26)[b]	.916	5993	1.89[d]
President's Budget	1955–73	.836 (.017) (49.93)[a]	.317 (.105) (3.00)[a]		.922	4725	1.75[d]
Congressional Appropriation	1955–73	.968 (.008) (109.67)[a]	.360 (.197) (1.82)[b]		.982	1980	1.65[d]
Expenditure	1955–73	.638 (.066) (9.73)[a]	.394 (.066) (5.95)[a]		.988	1860	1.45[c]

[a] Significantly different from 0.00 at the .01 level.
[b] Significantly different from 0.00 at the .05 level.
[c] Durbin's *h*.
[d] Durbin-Watson *d*.
[e] The 1954 observation is lost because of the manner in which the congressional appropriations equation is estimated.

parameter estimates that are both extremely sharp and extraordinarily plausible.

Having ascertained that the Reactive Linkage Model is not only plausible but also has a great deal of empirical support when analyzed on an equation-by-equation basis, we are ready to evaluate the model as a whole. Implicit in the formal version of the model is the assertion that it accurately portrays how a series of inputs is translated into a specific output—the U.S. defense expenditure total. As a result, the Reactive Linkage Model can be used to generate a series of historical forecasts of the expenditure total. The accuracy of the resulting forecasts will then provide one additional assessment of the estimated model.

To establish a minimum acceptable level of forecast accuracy below which it is possible to conclude that the theory implicit in the model does not actually tap the essential components of the defense expenditure policy-making process, the Reactive Linkage forecasts will be compared to those generated by the following no-change, naive model:

$$y_{5t} = y_{5t-1} + e_t. \tag{17}$$

According to Milton Friedman (1951, p. 109) this model is "the natural alternative hypothesis against which to test the hypothesis that the econometric model makes good predictions." Thus the forecasts generated by the no-change model can be used as a baseline for determining whether the forecasts generated by the Reactive Linkage Model are acceptable.

Table 3. Hypothesis Test Results

Hypothesis	t-ratio
H_1: $b_{11} > 1$	10.02[a]
H_2: $b_{12} > 0$	5.00[a]
H_3: $b_{13} > 0$	2.26[b]
H_4: $b_{21} < 1$	9.81[a]
$b_{21} > 0$	49.92[a]
H_5: $b_{22} > 0$	3.00[a]
$b_{22} < 1$	6.89[a]
H_6: $b_{31} < 1$	4.00[a]
H_7: $b_{32} > 0$	1.82[b]
H_8: $b_{51} > 0$	9.73[a]
$b_{51} < 1$	5.48[a]
H_9: $b_{52} > 0$	5.95[a]
$b_{52} < 1$	9.18[a]

[a] Significant at .01 level.
[b] Significant at .05 level.

Following standard econometric practice, I have generated the forecasts using the reduced form (Christ, 1966, p. 543). Instead of generating the forecasts for each of the current endogenous variables in the model, I have restricted the enterprise to the final reduced form equation because it alone provides a single equation representation of a recursive model and presents a substantively meaningful summary of the individual equations of the model. The Reactive Linkage Model's final reduced form equation can be obtained by substituting the services equation for y_{1t} in Equation 3, the result for y_{2t} in Equation 4, the result for y_{3t} in Equation 5, and finally the result for y_{4t} in Equation 6. Performing the suggested substitution yields the following final reduced form equation:

$$\begin{aligned} y_{5t} &= p_{51}x_{1t-1} + p_{52}x_{2t-1} + p_{53}x_{3t-1} \\ &\quad + p_{54}x_{4t} + p_{55}x_{5t} + p_{56}x_{6t} \\ &\quad + p_{57}y_{5t-1} + v_{5t} \end{aligned} \tag{18}$$

where

$$\begin{array}{ll} p_{51} = b_{11}b_{21}b_{31}b_{51} & p_{52} = b_{12}b_{21}b_{31}b_{51} \\ p_{53} = b_{13}b_{21}b_{31}b_{51} & p_{54} = b_{22}b_{31}b_{51} \\ p_{55} = b_{32}b_{51} & p_{56} = b_{51} \\ p_{57} = b_{52} & v_{5t} = b_{21}b_{31}b_{51}u_{1t} \\ & \qquad + b_{31}b_{51}u_{2t} \\ & \qquad + b_{51}u_{3t} + u_{5t}. \end{array}$$

The b_{ij} coefficients are the structural coefficients presented earlier.

The final reduced-form equation provides a substantively meaningful summary of the entire Reactive Linkage Model. The parameters, p_{ij}, measure the effect of the individual organizations upon the expenditure total as it moves from organization to organization. For example, the impact of the previous year's USSR defense expenditure total upon current U.S. defense expenditures is measured by p_{51}. As can be seen, the final reaction is a function of the services' initial reaction, the presidential cut, the congressional cut, and the Department of Defense's partial adjustment. No single organization bears final responsibility for the reaction to the USSR; instead, it is a product of the actions of all of the organizational actors making up the defense expenditure policy-making process. Such a characterization is consistent with Gray's (1971, p. 77) suggestion for understanding the defense policy-making process. Thus, the final reduced form equation makes explicit the notion that the defense expenditure decision can be viewed as one in which there is an initial reaction which is filtered through the remaining steps of the policy-making process to determine the magnitude, scale, and timing of the reaction.

Substituting the estimated b_{ij} coefficients from Table 3 as suggested in Equation 18 yields the following estimated final reduced form equation for the Reactive Linkage Model:

$$\begin{aligned} y^f_{5t} &= .725x_{1t-1} + 1.060x_{2t-1} + .346x_{3t-1} \\ &\quad + .192x_{4t} + .230x_{5t} + .638x_{6t} \\ &\quad + .394y_{5t-1} + 0 \qquad (19) \end{aligned}$$

$t = 1955, 1956, \ldots, 1973$

where y^f_{5t} is the vector of historical forecasts and y_{5t-1}, x_{1t-1}, x_{2t-1}, x_{3t-1}, x_{4t}, x_{5t}, and x_{6t} are vectors of the actual values of the predetermined variables. The naive model forecasts are generated from the following equation:

$$y^f_{5t} = y_{5t-1} + 0. \qquad (20)$$

In generating the forecasts, the values of the predetermined variables are updated after every point forecast. Furthermore, since there is no information with which to assign a value to the error term, it is assigned its expected value of 0.00. In short, the forecasts are single-period and non-stochastic.

The historical forecasts are displayed in Table 4 with the accuracy statistics presented at the bottom of each column.[15] As can be seen, the Reactive Linkage historical forecasts are more accurate than those of the naive model. Specifically, they have a smaller RMSE [\$4.0 billion to \$5.0 billion] and are more highly correlated with the actual expenditure values [.98 to .96]. While there is not a statistically significant difference between the two correlations, the magnitude of the difference between the RMSEs indicates that the Reactive Linkage Model's forecasts are more accurate than those of the naive model.

When the previously reported empirical results are combined with the inability of existing models of the U.S. defense expenditure policy-making process (e.g., *Arms Race* and *Organizational Politics*) to generate forecasts which are uniformly more accurate than those generated by a no-change naive model (Ostrom, 1977), we may conclude that the Reactive Linkage Model represents an improvement over existing models. Therefore, from the empirical results reported in this article, I would argue that the Reactive Linkage Model, and hence the idea of combining the key elements from competing explanations, represents a positive step forward in the study of U.S. defense expenditure policy making.

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[15] The two accuracy statistics are the root mean square error (RMSE) and the correlation between the actual and predicted values (r_{AP}).

Table 4. Actual and Forecasted Values of U.S. Defense Expenditure Total, 1955–73

		Forecasted Expenditure Total	
Year	Actual Expenditure Total	Reactive Linkage Model	No-Change Naive Model
1955	35533	32520	40336
1956	35791	37059	35533
1957	38438	38040	35791
1958	39063	38025	38438
1959	43573	34121	39063
1960	41215	38505	43573
1961	43227	37346	41215
1962	46815	45329	43227
1963	49973	51486	46815
1964	49760	50731	49973
1965	45973	47754	49760
1966	54178	55945	45973
1967	67547	64012	54178
1968	77373	73720	67547
1969	77872	85244	77373
1970	77150	78776	77872
1971	75546	73470	77150
1972	75084	71377	75546
1973	76435	82420	75084

War, Presidents, and Public Opinion

Even though the Reactive Linkage Model treats the U.S. defense expenditure policy-making process as if it were completely stable, there can be little doubt that it is susceptible to changes or shocks over time. That is, it is likely that certain discrete events have had a short-term impact on U.S. defense expenditure policy making. Not only does previous work suggest that the budgetary process is affected by discrete shocks from the domestic and international environments (Davis, Dempster, and Wildavsky, 1974) but also factors such as the occurrence of war, a change in the party of the president, and the development of a large bloc of negative public opinion are often suggested as important determinants of U.S. defense expenditure policy (e.g., Russett, 1972; Crecine and Fischer, 1973; Mueller, 1973). To this point in the development of the Reactive Linkage Model such contingencies have been ignored. But assuming that the model is a reasonably accurate representation of "policy making as usual," we can use it as a framework within which the effects of these or similar shocks can be studied.

To do so I assume that the presence of a war, a change in presidential party, and negative public opinion can be viewed as discrete shocks or qualitative changes in either the domestic or international environments which may temporarily alter one or more of the policy-making rules. The definitions and operationalizations of these three variables are presented in Table 1.[16] Rather than simply correlate war, presidential party, or public opinion with the various organizational decisions, I have entered these factors multiplicatively into each policy-making rule to see if they induce a behavioral change (Davis, Dempster, and Wildavsky, 1974). For example, to determine the effect of war, z_{1t}, on congressional appropriations, the following equation is estimated:

$$y_{3t} = (b_{31} + a_{31}z_{1t})y_{2t} + (b_{32} + a_{32}z_{1t})x_{5t} + u_{3t} \qquad (21)$$

where b_{31} and b_{32} represent the impact of y_{2t} and x_{5t} on y_{3t} in the absence of war while $(b_{31} + a_{31})$ and $(b_{32} + a_{32})$ represent the impact of y_{2t} and x_{5t} on y_{3t} in the presence of war. If a_{31} and/or a_{32} are different from 0.00, there is evidence that the parameters of the policy-making rule are temporarily altered as a result of the existence of war. Furthermore, the sign and magnitude of each a_{ij} provide a measure of the change in behavior brought on by changes in either international or domestic environments.

To maintain reasonable degrees of freedom, I have entered the qualitative variables into each of the four equations one at a time. In this way it is possible to see if the three shocks separately have an impact on the various steps of the defense policy-making process. The estimated a_{ij} coefficients and associated t-ratios are presented in Table 5. As can be seen, the defense policy-making process is not uniformly affected by these three shocks; instead, the discrete events have an impact only on certain stages of the policy-making process and the responses of the different stages are distinctive. With the exception of the services' request which already includes a war-related variable, the existence of war does not have a significant impact on any of the policy-making rules. The presence of a high proportion of negative opinion has a dampening effect on the services' request and the congressional appropriation. This suggests that when unified in their opposition, the people do have an impact on U.S. defense policy making. Finally, the party of the president affects only the president's budget; specifically, Democrats tend to cut more from the services' request while simultaneously being more responsive to the ratio goal than Republicans. It should also be noted that the expenditure policy-making rule does not appear to be affected by the existence of discrete shocks.

These results seem very plausible in the context of the U.S. defense expenditure policy-making process and hence provide additional support for the Reactive Linkage formulation. In addition, they shed some light on how the impact of discrete changes in the international and domestic environment can be studied. The present analysis can easily be extended to different shocks or to different operationalizations of the same shocks as long as one is willing to assume that the Reactive Linkage Model provides a reasonably accurate representation of the U.S. defense expenditure policy-making process.[17]

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[16] With the exception of the public opinion variable (z_{3t}), the operationalization of the binary variables is very straightforward. A problem arises with the public opinion variable because questions relating to defense spending were not asked in 1954, 1956, 1958, 1959, and 1961–67 (Russett, 1972). My strategy is to use the variable only when the appropriate questions were asked. If the question is asked, and if "too much" is greater than "too little" plus "about right" (i.e., ignoring the no opinion responses), the variable is coded 1; otherwise it is coded zero.

[17] Another way in which the dummy variable approach can be used is to identify the sources of

Conclusion

This article began by noting that a number of partially contradictory and partially complementary explanations are vying for acceptance as the most accurate explanation of U.S. defense expenditure policy making. By exploiting the fact that these models complement one another, and by resolving some of their inconsistencies through the development of a synthesis of the *Arms Race* and *Organizational Politics* explanations based on the suggestions of scholars usually associated with the *Bureaucratic Politics* perspective, this article provides a new focus for the current debate. Instead of arguing over which model is better, I have focused the enterprise on the integration of the key elements of the contending explanations into a single explanation.

What do the results of this study suggest? First, the specification of the formal model is very plausible. Not only does the Reactive Linkage Model reconcile the substantive criticisms of the *Arms Race* and *Organizational Politics* models by integrating their basic components into a single model, but also the resulting synthesis is consistent with the views of a number of prominent proponents of the *Bureaucratic Politics* approach. Second, the estimated version of the model provides an accurate representation of the pattern of U.S. defense expenditure policy making from 1954 to 1973. The individual equations account for a large portion of the total variance with parameter estimates that are both sharp and plausible. Taken together, the equations of the model generate more accurate historical forecasts than those of the naive model. Furthermore, the Reactive Linkage Model, which incorporates the key elements from the *Arms Race* and *Organizational Politics* models into a single explanation, performs better than either model has previously done separately. Finally, working on the assumption that the Reactive Linkage Model is an accurate representation of the defense expenditure policy-making process, I have investigated the impact of a number of exogenous shocks. The results of this analysis are both interesting and instructive. The dummy variables used to represent the shocks suggest that different events have an impact on only some of the steps in the policy-making process and the steps which are so affected vary from event to event.

The Reactive Linkage Model, as presently constituted, is by design a compromise between the existing explanations which is weighted heavily in favor of the simpler and more formal explanations. Further research may therefore be directed at increasing the complexity of the

Table 5. The Impact of War, President's Party, and Negative Public Opinion on the Reactive Linkage Model Policy-Making Rules

Policy-Making Equation	War a_{i1}	War a_{i2}	War a_{i3}	President's Party a_{i1}	President's Party a_{i2}	President's Party a_{i3}	Public Opinion a_{i1}	Public Opinion a_{i2}	Public Opinion a_{i3}
Services' Request[b]	a	a	a	.02 (.21)	–.09 (.05)	–.91 (1.41)	–.12 (1.30)	–.19 (.37)	–.29 (.37)
President's Budget[b]	.01 (.21)	.06 (.12)		–.06 (1.64)	.37 (1.10)		.03 (.81)	–.18 (.54)	
Congressional Appropriation	–.02 (.95)	.31 (.71)		–.01 (.21)	.44 (.82)		–.05 (3.21)	.13 (.32)	
Expenditure	.62 (.36)	–.58 (.33)		.15 (.83)	–.15 (.83)		.10 (.55)	.09 (.54)	

[a] War-related variable is already included in services' request equation.

[b] Lagged values of three variables used for these equations.

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cutting and spending moods. Assuming that one omits x_{5t-1} from Equation 4 and in its place includes dummy variables which represent the sources of cutting and spending moods, C_t and S_t respectively, the congressional policy-making rule can be rewritten as:

$$y_{3t} = (b_{31} + b_{32}C_t + b_{33}S_t)y_{2t} + u_{3t}.$$

One would expect b_{31} to be marginally less than 1.00, b_{32} to be less than 0.00, and b_{33} to be greater than 0.00. Once the policy-making rule has been specified in this form, all that remains is to determine the specific shocks which might precipitate cutting and spending moods. Using various pairs of z_{1t}, z_{2t}, and z_{3t}, I could not find a combination which yielded the desired results. It is possible that subsequent research might uncover shocks which are plausible sources for these moods. If so, the result would be a more intuitively appealing version of the congressional policy-making rule.

explanation in order to more closely approximate the concerns of the *Bureaucratic Politics* theorist. In this regard, the Reactive Linkage Model can be modified incrementally in response to specific criticisms and/or observations of students of defense policy making. The current model is but a first step towards integrating existing explanations of U.S. defense expenditure policy making.

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The Corruption of a State*

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This article presents a theory of corruption which unifies the moral, political, economic and social causes and patterns of corruption in one theoretical framework. The theory is constructed from the scattered insights about the "corruption of the body politic," building in particular upon the work of five theorists–Thucydides, Plato, Aristotle, Machiavelli and Rousseau. Corruption is defined as the moral incapacity of citizens to make reasonably disinterested commitments to actions, symbols and institutions which benefit the substantive common welfare. This extensive demise of loyalty to the commonwealth comes from the interaction of human nature with systematic inequality of wealth, power and status. The corruption of the polity results in certain identifiable patterns of political conflict and competition. The central feature of these patterns is the emergence of quasi-governmental factions and an increasingly polarized class system. The politics of the factions leads to an undermining of the efficacy of the basic political structures of the society and the emergence of systematic corruption in all aspects of political life. The theory advanced in this article identifies several crucial prescriptions to stave off the tendency towards corruption. Among these are an extension of maximum substantive participation by all citizens in all aspects of political life and a stringent control over all sources of great or permanent inequality in the polity.

The disintegration of ordered arrangements of life is a central problem of politics. When the daily interactions among people and institutions no longer provide normal opportunities for the exercise of integrity, personal right, or fulfillment, political theorists can no longer ignore the decay. As people proclaim the "twilight," "decline," or "crisis" of every major aspect of our culture, we must try to comprehend the nature of political disintegration.

The explanations for the increased disordering of human lives have tended to divide along three lines–institutional, moral, and economic. The institutional approach argues that outmoded social and political structures can no longer provide for a population whose size, values and expectations have radically changed since they were instituted. The moral explanation sees certain undesirable moral changes result in a collapse of traditional moral disciplines, and sees people without self-discipline or altruism placing unwarranted demands upon institutions. Finally, the economic interpretation argues that unequal economic and power distributions have generated forces which have alienated the people and lead to the social breakdowns. In this paper I will present the theory of corruption as an alternative account of the decay of trust, loyalty and concern among citizens of a state.

While in contemporary usage "corruption"[1] usually means the betrayal of public trust for individual or group gain, the technical notion of the "corruption of the body politic" has a long and impressive history in both political philoso-

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[1] *The Oxford English Dictionary, Compact Edition* (1971, Vol. 1, pp. 566–67) cites a number of definitions which are relevant to the theory and reflect the older Latin and French usage. The first definition of "to corrupt" is "to turn from a sound into a unsound impure condition." The fourth definition specifies another aspect, "to destroy or pervert the integrity or fidelity of a person to his discharge or duty; to induce to act dishonestly or unfaithfully; to make venal; to bribe." The notions of decomposition and degeneration apply to many areas but the most prominent is the corruption of the customs, habits and morals of individuals and societies. The second basic category of meaning under the word "corruption" is "moral." Moral corruption can apply to "agents, practices, institutions, natures, customs, officials" and almost every aspect of human activity where moral choice involves the possibility of acting in one's own interests or being loyal to a public trust, law or another's welfare.

phy and polemics.[2] The decay of the moral and the political orders are phenomena which political theorists have constantly had to confront. In this article I assume that, while historical situations change, there is a continuous tradition of rational reflection upon such problems and that the results of this reflection need not be limited to the comprehension of a particular era. I further assume that the decay of political orders are not incommensurable events.[3]

The arguments about corruption are scattered throughout the western political tradition but a coherent theory of corruption has never been fully articulated. "Standing on the shoulders of giants," I have found the insights of five theorists–Thucydides, Plato, Aristotle, Machiavelli and Rousseau–fruitful enough to enable me to construct an independent theoretical account of the decay of a political order.[4] This theory of corruption is worthy of serious consideration and further study because it makes a number of significant contributions to our understanding of politics.

First, the theory establishes a clear link between the moral and social prerequisites of a just and stable state and structural inequality. While taking "conservative" moral concerns seriously, it does not divide them from more structural concerns. In this, it provides a non-Marxist framework to comprehend the relations among inequality, classes, civic morals, interest group/factions and the structures of government. *Second,* the theory presents a suggestive critique of "liberalism" by arguing that several of liberalism's normative and psychological assumptions are insufficient either to justify or sustain a just, equal and stable state. *Third,* the theory complements and enriches many of the existing critiques of pluralism. By identifying certain types of interest groups as factions, it provides a set of moral and social insights about the limits of pluralism and gives an overarching moral coherence to the whole critique of pluralism. *Fourth,* the theory provides a suggestive and comprehensive model which explains certain prevalent patterns of politics and synthesizes a wide variety of insight and empirical work. It is especially relevant in its redefinition of political decay and stability and its presentation of alternative policies to build a stable polity. Some of the relevant empirical work encompasses the function of co-optation, the role of the military in a civilian state, the political powerlessness of the poor, the effects of political participation, the role of political socialization, and the importance of political symbols and acquiescence. *Finally,* without resorting to either reaction or revolution, the theory provides limited and realistic prescriptions for ameliorating one of the recurring problems of politics–the corruption of a state.

At this point I will briefly summarize the theory and then examine its tenets in more detail. The theory of corruption involves the following propositions:

1. Certain patterns of moral loyalty and civic virtue are necessary to maintain a just, equal and stable political order. The privatization of moral concerns and the accompanying breakdown of civic loyalty and virtue are the cardinal attributes of a corrupt state.

2. Extensive inequality in wealth, power and status, spawned by the human capacity for selfishness and pride, generates the systematic corruption of the state. Members of the upper classes sacrifice their basic civic loyalty to gain and maintain their positions and the established inequality undermines the loyalty and substantive welfare of the general citizenry.

3. This change in the moral quality of life of the citizen, coupled with inequality, generates factions. Factions are objective centers of wealth, power, police and policy which, by their own dynamics, usurp vital governmental and political functions. Factional politics involves the systematic attempt to corrupt public agency and law. Membership and practice in the factions changes the moral character of persons, undermines their loyalty to the community and

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[2] A detailed analysis of the various forms of the theory can be found in Pocock (1975). Bailyn (1967) and Wood (1969) analyze its form and importance in the period of the American Revolution.

[3] Wolin (1960, pp. 1–28).

[4] This article is *not* an historical survey of particular theorists. Nor is it an explication of the theory of corruption which might underlie all the various theorists. Rather, my formulation of the theoretical model draws upon, but is not necessarily identical to, the insights culled from them. Even with this caveat, one may ask why I selected these five theorists upon which to base my theory. Quite simply, I found that these five provided all of the initial historical and theoretical analysis necessary to develop the full theory.

Because I base my theory on these theorists, it is necessary to note the following. While the theorists' views of the just state may vary depending upon assumptions about equality, property and human nature, their portrayal of corruption is almost uniformly the same. But underlying their notions of corruption, one will find two basic differences. These concern their differing notions of a philosophy of history and human equality. The cyclic theory of history, found in Plato and Machiavelli, or more modern progressive notions of history are not addressed in this paper. I do not believe such historical theories are necessary to the theoretical model, and the analysis, as I develop it, may actually count against holding a theory of history. The nature of equality will be discussed under "The Cause of Corruption."

encourages radical selfishness or limited loyalty to factions.

4. The factional conflict and continued inequality extend corruption across the entire citizenry. Violence increasingly becomes the dominant substratum of all relations, and political discourse is reduced to transparent rationalization. Public office, law and adjudication become tools of faction and class. The disenfranchised populace and the upper classes become increasingly polarized. Demagogic factional politics, sporadic uprisings and cooptation mark political relations as the society moves in a restless cycle from aborted attempts at "restoration" and "reform" to increasing alienation, violence and institutional anarchy.

5. The socialization of education, family life, religion and the military also sustains communal values and loyalty, sometimes even after the corruption of the political process. The final corruption of the state involves the failure of the citizenry to support these primary structures voluntarily.

The Corruption of the State

Moral corruption is the loss of a capacity for loyalty. Individual moral life becomes progressively privatized and self-interest becomes the normal motive for most actions. The privatization of moral concerns changes the moral calculus of the society. The self-interested contract becomes the normal social relation, and any arrangement becomes rational through which an individual gains more from another than is given. The primary attitude among citizens is wary competition to preserve what one possesses and to gain more if possible.

Societal or state corruption involves the moral incapacity of citizens to make disinterested moral commitments to actions, symbols and institutions which benefit the common welfare. A slightly weaker definition is that citizens are unable or unwilling to do anything which does not bring them sensual gratification, money or security. Corruption leaches the trust and fraternity from the social life of the state. Acceptable communal answers to problems such as marriage or property defense become problematic. Mistrust and latent competition among individuals change the everyday moral universe and citizens no longer can or wish to sustain, at some cost to themselves, certain patterns of committed relations to other citizens.

Loyalty is the focus of this theory because it is the constitutive moral and psychological attribute of the minimum civic virtue necessary to sustain the symbols, laws and institutions of the state. Josiah Royce in *The Philosophy of Loyalty* defines loyalty as

> *the willing and practical and thoroughgoing devotion of a person to a cause.* A man is loyal when, first, he has some *cause* to which he is loyal; when, secondly, he *willingly* and *thoroughly* devotes himself to this cause; and when, thirdly, he expresses his devotion in some *sustained and practical way,* by acting steadily in the service of his cause.[5]

The importance of loyalty flows from its central role in moral autonomy. Moral autonomy requires a self-conscious capacity to rationally and emotionally affirm impersonal values, concrete relations and symbols which embody these relations and values. Without a capacity for loyalty to these "causes," people could not exercise the self-discipline necessary to override self-interested desires or work for other people's or even their own welfare. The exercise of duty to ourselves and others flows from the capacity for loyalty. Royce argues that people cannot really be loyal to their own desires; while these desires might form a random or Hobbesian hierarchy of impulses, they do not form an impersonal coherent whole, a personal character. Desires of themselves define the content of selfishness, not selfhood. The capacity for loyalty enables people to order their beliefs and their lives and create selfhood and virtue in their strict sense.[6] Thus, loyalty constitutes the absolutely necessary, but by no means sufficient, moral and psychological prerequisite of moral autonomy and civic virtue.

Civic virtue is the dutiful activity which arises from a reasonably disinterested commitment to the well-being of other citizens and the institutions which provide for the basic needs and integrity of all citizens. Unlike simple consensus or opinions, the moral beliefs and actions of civic virtue are exceptionally stable and possess a degree of psychological autonomy as opposed to interests and inclinations. Civic virtue leads to actions not merely intended to maintain stability but also to achieve justice even if this goal involves some sacrifice. The capacity for disciplined sacrifice which flows from this type of moral commitment enables any real-life state to resolve its myriad conflicts with a minimum of violence and a maximum of justice.

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[5] Royce (1969, Pt. 2, p. 861). Royce goes on the claim that loyalty to loyalty is sufficient to provide the content of a strong humanistic ethic. While not wishing to defend Royce's derivation of ethics from loyalty, I do agree that a person cannot be moral or virtuous in any meaningful sense without the exercise of loyalty.

[6] Royce (1969, Pt. 2, Chs. 1, 5, esp. p. 886 ff.). This impersonality of self resides in the laws, personal rules of conduct and relations which exist independent of an individual's personal desires and needs and form a coherent and predictable moral personality.

A classic case of this sort centers upon citizens' willingness to risk their lives in political situations. A litmus test of corruption has always been the ability of a country to mobilize its citizenry and militia to defend itself effectively against tyrants and foreigners.[7] The willingness of citizens actively to support the laws as opposed to their willingness to reject them, drastically affects the overall stability of the society. The resolution to resist the law, although made in the crucible of economic and social flux, is, in the end, a personal moral decision. In his analysis of the Florentine conspiracies against the usurption of the Duke of Athens, Machiavelli recounts, "Many citizens, of every sort, determined to lose their lives or to have their liberty again."[8]

Civic virtue requires not only loyalty but also disinterestedness and personal allegiance to the common good. Consequently totally selfish persons are totally corrupt in that they possess no loyalty, no disinterestedness and no commitment to the common good. Loyalty, however, is prior, for without it a person could be neither disinterested nor committed to the community.

Civic virtue depends upon the extension of loyalty to the communal structures of the society. Habits, customs and spontaneous empathy with other citizens give daily content to active loyalty. This active civic loyalty is not simply emotion-dominated patriotism. Rather, all true loyalty requires reasoned reflection before the beliefs are affirmed. Loyalty also underlies committed actions for the disinterested welfare of people in the family, church or fraternal organizations.

Loyalty of itself, however, has never been a sufficient barrier against corruption. The classic problem arises with people who are loyal to morally hideous values or a reprobate group—a loyal Nazi or Mafia member. Insofar as such individuals are reflective and disinterested in commitment and dedicated in belief, they cannot be said to be corrupt. Insofar as the policies of the faction to which they are loyal undermine the substantive welfare of the citizenry, they are corrupt, but only in this more limited sense.

The Cause of Corruption

It is often a temptation to dismiss corruption as a fact of life rooted in flaws of human nature and to analyze most acts of corruption as isolated individual acts. However, there is unanimous agreement among the theorists that the source of systematic corruption lies in certain patterns of inequality.[9] In a limited sense most corruption requires individual moral choices and depends upon the human capacity for avarice and evil; nevertheless, the corruption of a state results from the consequences of individual human nature interacting with systematic and enduring inequality in wealth, power and status. Under such inequality certain groups of individuals have de facto or legally sanctioned priority of access to wealth, power and status.[10]

It should be clear that not all corruption necessarily occurs as a result of inequality. Nor will the end of all systematic inequality result in the elimination of all corruption. Corruption, however, can be viewed on a spectrum ranging from random individual acts through increasingly widespread corruption to the point where the citizenry, both in and out of government, engage in politics permeated by corruption. Certain patterns of inequality are the main generators of such increasing corruption, as opposed to random individual acts.

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[7] Machiavelli (1965, *Discourses*, Bk. 1, Chs. 4, 43; Bk. 2, Chs. 10, 12); Rousseau (1964, *Du contrat social*, Bk. 3, Ch. 15).

[8] Machiavelli (1965, *History of Florence*, Bk. 2, Ch. 36).

[9] Plato (1957, 421d–422b; 547a–53e); Aristotle (1962, Bk. 2, Ch. 7; Bk. 5, Ch. 2); Machiavelli (1965, *The Prince*, Ch. 9; *Discourses*, Bk. 1, Chs. 2–5); Rousseau (1964, *Discours sur l'inégalité*, pp. 171, 174–86, esp. pp. 187–91; *Discours sur l'economie*, p. 258).

[10] All the theorists acknowledge the innate capacity of human beings for selfishness and evil. Rousseau designates it in his distinction between "amour de soi" and "amour propre" (1964, *Discours sur l'inégalité*, p. 164 ff.). For Machiavelli even the best of individuals can be "bribed" by a "little ambition" and "avarice" (1965, *Discourses*, Bk. 1, Chs. 3, 42). He also points out that "moreover, human wants are insatiable, since man has from nature the power and wish to desire everything" (1965, *Discourses*, Bk. 2, Preface) and "republics go to pieces" when men "climb from one ambition to another" (1965, *Discourses*, Bk. 1, Ch. 46). Plato identifies the avaricious element of the soul as the most dangerous element in the triad of human nature. The corruption of the city and of human nature is defined by the increasing dominance of the avaricious and selfish part of the soul (1957, 444a–445d, 547b–587d, esp. 577d–587d); Aristotle argues that "men are always wanting something more and are never contented until they get to infinity" (1962, Bk. 2, Chs. 7, 8).

The focus is upon equality because of its relation to the common good. As the words imply, the common good at least partially entails goods which are equally common to all citizens. Given human selfishness and the normal conflicts of a state, maintaining the common good requires some loyalty to other people and to the policies and institutions which guarantee the common good. Loyalty declines under the pressure of inequality as individuals pursue purely selfish activities or act from limited loyalties to factions. Both these types of activities seek to benefit individuals or groups unequally, regardless of the consequences for the equal distribution of common goods. The methods of seeking such benefits extend the corruption of the people and undermine the structures designed to provide for the common good.

The theory, however, never assumes that all inequality is unjust and corrupting. The practical requirements of a society necessitate some inequality in the economic and political realms. The theory distinguishes just and reasonable inequality from that which generates corruption. The non-corrupt state guarantees certain basic forms of substantive economic, juridical and political equality, but it does not require absolute equality in all aspects of life. Any reasonable inequality can be justified as long as it contributes to the substantive commonweal or at least does not endanger citizens' substantive freedoms.

Practical inequality in the ownership or control of wealth can be justified on two grounds: (1) the limited but legitimate claims of distributive justice, (2) the need to generate surplus wealth to finance the government and the common good. The functional specialization necessary for the maintenance of a society above the subsistence level, the natural unequal distribution of talents and interests, as well as the exigencies of a money economy, generate some economic inequality. In turn, the inequality generates the surplus necessary to finance the state.[11] Economic inequality, however, must never develop to the extent where it threatens the integrity of law or government. Hereditary sources of great wealth cannot be tolerated and any significant wealth must be controlled by law. All citizens must have their economic integrity guaranteed. Redistributing property to provide all citizens with a livelihood, progressive taxation, inheritance taxes, sumptuary laws, excise taxes, and minimization of foreign trade have all been proposed as means of warding off the dangers of wealth in a state. Machiavelli argues that it was the relative economic equality of the German city republics which gave them the strength to maintain their freedoms against superior forces.[12]

Political and social subordination and the accompanying inequality in power are the preeminent forms of inequality necessary for the state. The laws must be legislated, promulgated, enforced and administered, and these activities require that unequal power and respect be given to some individuals. Spontaneous obedience is the heart of a just, equal and stable state. Yet without qualifications, obedience to the laws may bring stability but certainly not justice or equality. Consequently there are two sets of qualifications about the quality of laws and officials in a non-corrupt state.

First, the laws must apply equally to all citizens and be fairly administered. The laws must be designed to benefit all citizens equally and not one particular group. Finally, those who make and administer the laws must be equally subject to them.[13]

Second, the spontaneous acceptance of government assumes that government officials are loyal to the common welfare. It also assumes that their dedication is augmented by talent and competence. The destruction of the Athenian forces at Syracuse under the well-meaning incompetence of Nicias and the unprincipled ambition of Alcibiades, demonstrates the dangers of one set of qualifications without the other.[14] Justified hierarchical inequality presumes talent, dedication and virtue in those to whom office is entrusted.

In the real world of politics it is extremely difficult to maintain any equality or to maintain both competent and virtuous individuals in office. The realm of politics tends to attract talented and ambitious citizens regardless of their civic virtue; the weaknesses of human nature combined with the temptations to abuse official authority necessitate some more substantive limits upon political power.[15] Substantive citizen participation is the best way to accomplish this. Such participation allows all citizens in office and offsets inequalities in wealth and status, and it maximizes the responsibility and virtue of the majority of citizens while limiting the opportunities for misuse of power. There are a number of political strategies to ensure limits upon inequality and corruption. These are: maximum citizen participation in elections, offices, and the armed forces; constant rotation of representative and bureaucratic governmental offices; a citizen army; a maximum number of elected officials; open civilian juries; the minimization of the hereditary element in any positions; and extension of the meritocratic award of office.[16]

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[11] Plato (1957, 369b–374e); Aristotle (1962, Bk. 2, Chs. 2–7); Machiavelli (1965, *Discourses*, Bk. 1, Ch. 1); Rousseau (1964, *Du contrat social*, Bk. 3, Ch. 8).

[12] Machiavelli (1965, *Discourses*, Bk. 1, Chs. 37, 55); Rousseau (1964, *Du contrat social*, Bk. 2, Ch. 11; *Projet pour la Corse*, pp. 904–06, 930–37; *Considérations sur le Pologne*, pp. 972–75, 1003–12).

[13] Rousseau (1964, *Du contrat social*, Bk. 2, Chs. 3–6, 11, 12; Bk. 3, Ch. 10; *Discours sur l'economie*, pp. 247–60, esp. pp. 252–53); Machiavelli (1965, *Discourses*, Bk. 1, Chs. 7, 8, 24, 58; *History of Florence*, Bk. 3, Ch. 5).

[14] Thucydides (1934, Bk. 6, Chs. 18, 19; Bk. 7, Chs. 22, 23); Machiavelli (1965, *Prince*, Chs. 14–16; *History of Florence*, Bk. 3, Ch. 23).

At this point in the formulation of the theory I side with Aristotle, Machiavelli and Rousseau in their insistence that just equality demands open and meaningful participation by the widest possible body of citizens. This is probably the most relevant difference among the sources of the theory. For Plato, the stasis of corruption derives from the failure to match the specialized jobs of government with the innate, invariant and hierarchical distribution of talent and knowledge in the society. Rousseau, and to a lesser extent, Aristotle and Machiavelli argue that most citizens both are capable of participating in the political realm and must participate to adequately achieve virtue and guard against the onslaught of corruption. Although this difference sets the theorists apart on most issues, it does not affect the moral and political consequences of corruption. The essence of corruption remains the same: the decline in the ability and willingness of the citizens to act spontaneously or disinterestedly to support other citizens or communal institutions.

The relation between inequality and corruption centers upon the moral relations of people in an unequal state and the patterns of politics which they engender. There are two types of inequality which corrupt the state: permanent or massive inequality in wealth, and exclusionary inequality in political power and authority.

The corruption stemming from economic inequality is the most insidious and pervasive. It begins with the moral life of the seeker of great riches. To expend time and energy in amassing wealth not only requires talent but also a peculiar moral perspective in which most emotions and talents are honed exclusively towards the satisfaction of personal desires. The attainment of great wealth also requires the use and organization of people and resources. Other citizens must be viewed primarily as a means to a private end. This instrumentalization of human relations and the habit of using citizens for one's own ends slowly erode the individual's disinterested commitment to their welfare. Even their shared community of aims begins to change. It becomes rational for the rich person to worry more about fellow citizens' envy rather than their lack of equality. It becomes morally rational to subvert the government to protect one's position and ensure that no one else can use the government against one's wealth.[17]

Selfish individuals do not of themselves destroy the state and can actually help if their pursuit is tied to the public glory and aggrandizement. Machiavelli's chronicle of Lorenzo and Cosimo de Medici's success at holding Florence together with corruption, talent and prosperity shows the accomplishments and the limits of this idea. At their death, as was the case with Pericles in Athens, the corrupt system fell apart without their singular personal strengths to hold it together. In the long run such a system destroys the remnants of loyalty and will collapse.[18]

The other corruption-generating inequality begins once citizens are denied participation in government and authority except on the basis of exclusive criteria such as land, title, or party. All governments tend to act as dangerous self-interested factions, and members of the government try to assert their long-term prerogative to rule regardless of the initial form of government. All corrupt governments move toward hereditary power. The result might be a simple heriditary nobility or a de facto hereditary state such as a self-perpetuating one-party state or a merchant oligarchy as in Venice. Even elected officials of a democratic republic will move in this direction, as Machiavelli shows in his study of the Decemvirate in the Roman Republic.[19]

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[15] Thucydides (1934, Bk. 3, Ch. 10); Plato (1957, 521a); Aristotle (1962, Bk. 5, Ch. 8).

[16] The classic statement of most of these ideals resides in Rousseau's *Considérations sur le Pologne.* This underrated work, along with *Projet de constitution pour la Corse,* represents Rousseau's final synthesis of his Platonic, democratic and republican heritage. They provide a systematic statement of the classical republican ideal with a strong democratic tinge.

[17] Plato (1957, 550c–556e); Machiavelli (1965, *Discourses,* Bk. 1, Chs. 2, 5, 46; *History of Florence,* Bk. 3, Chs. 5, 8–11); Rousseau (1964, *Discours sur l'inégalité,* pp. 171–74, 177–82, 202–04).

[18] Thucydides (1934, Bk. 2, Ch. 7); Machiavelli (1965, *History of Florence,* Bk. 7, Chs. 1–6); Rousseau (1964, *Discours sur l'economie,* pp. 252–62).

Once a group gains exclusionary control of the government and authority—or, at least, has first priority in access—it becomes morally rational to try to maintain this power position. A diverse variety of claims are used to justify this control. Among these claims are: a group has a greater interest in the state and more time because of its wealth; a group is better trained and more experienced as in a hereditary nobility; a group has more talent and commitment as in a one-party state. Over time these claims reduce to nothing more than rationalizations to maintain power. It is feasible that with strong socialization, discipline and tradition an exclusionary group would not unduly tyrannize the population and might rule in the common interests of all the citizens. Yet, even when the elite performs well, the people's perception of permanent inequality will undermine their loyalty.[20] More likely, the elite socialization will slip and the scions of the ruling group will ultimately use the government for their own aggrandizement or act to maintain their own prerogatives whenever they feel threatened by the claims of the citizenry. Only significant and guaranteed participation by the citizenry can ward off these tendencies.

Factions

One of the root meanings of corruption is literally "to break into many pieces." This is the fate of a corrupted state. The community was never expected to be homogeneous, but a just, equal and stable society needs a minimum set of rational and emotional commitments to the common welfare and the sustaining structures of the state. These social commitments enable the rifts and conflicts over inequality and human mischievousness to be reconciled peacefully and enable a community to defend itself, to provide social answers to basic human problems and to encourage gradual reform of injustice. The factions into which the community breaks destroy the loyalties which sustain this community.

These are not the simple factions of Madison's or economists' dreams. Neither Madison nor ersatz Madisonians account for the dangers wrought in the community by a true faction. *The History of Florence, The Discourses on the First Decade of Titus Livy* and *The Peloponnesian War* are, if anything, historical rejections of the naive Madisonian and pluralist thesis that the conflict among factions will result in the prevention of tyranny or the long-term maximization of all citizens' welfare.

The factions are objective centers of power; they encompass families, corporations, unions, governmental bureaucracies and similar associations; their hallmarks are autonomous power and internal cohesion sufficient to distort government and to provide semigovernmental services for their dependents. They are capable of directing people and resources for the pursuit of their own goals against opposition, legal or otherwise. Their membership can possess loyalties, traditions and goals of their own and literally become "laws unto themselves" with quasi-official private police forces to rule their domains and resist the state if necessary.[21]

Commanding power, money and security, the factions develop dependents who rely upon them for welfare and services. In some ways the factions develop their own laws for their members. To protect and earn members' trust, they must often suborn government officials and gain privileges from the law. It becomes rational to work systematically to corrupt the government in order to maintain the faction's own basis of power.

As moral phenomena, factions put limited private interests before the public responsibilities of the citizens and the government, and they socialize the citizens into this framework. They engender citizens whose economic need and dependency are turned into quasi-self-interested loyalty to the faction, not to the community. Broken from basic loyalties to others in the community, the member of a faction begins to view law as a tool to further factional interests. Although little loyalty exists among atomized selfish people, factions try to engender some loyalty in their members simply to strengthen themselves. Since "very rarely do men understand how to be altogether bad or altogether good," the totally corrupt individual is a rarity.[22] Often the "love of a party," or ties of friendship or affection for a leader will create some tenuous loyalty among partisans. Rousseau suggests that although the "individual will" supplants "the communal self" in a corrupted person, these "small societies" will act as sort of a pseudo "general will" for the individual.[23] A person might even come to internalize the goals of a faction and become loyal in the fuller emotional and moral sense. At this point the loyalty has been narrowed to the concerns of the faction, not the common good. Civic loyalty is undermined and either destroyed or transferred to lesser associations. By narrowing the focus of loyalty, the faction also undermines disinterestedness by limiting the morally significant individuals to the faction's membership.

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[19] Rousseau (1964, *Du contrat social,* Bk. 3, Ch. 10); Machiavelli (1965, *Discourses,* Bk. 1, Chs. 40–44).

[20] Aristotle (1962, Bk. 5).

[21] Machiavelli (1965, *History of Florence,* Bk. 3, Ch. 5; Bk. 7, Chs. 1, 2, 23; *Discourses,* Bk. 1, Ch. 46); Rousseau (1964, *Du contrat social,* Bk. 3, Chs. 10, 11, 15).

[22] Machiavelli (1965, *Discourses,* Bk. 1, Ch. 27).

In gauging the loyalty which exists in such factions, a distinction can be made between groups which evolve from primary emotional and ethical loyalties, such as clans, families and churches, and those which develop from more institutionalized corporate groupings. This second set of corporate groups might be further divided into consciously self-interested factions such as corporations and unions or more public or governmental factions such as rogue bureaucracies, secret liberation societies or political parties. In both cases, but especially that of the public groups, there might exist a complex mix of private, organizational and public interest motives in each participant. Thus official vigilantism might involve individuals seemingly committed to communal loyalties but using the corrupted methods of factions. This last situation is the most complex and dangerous because individuals with authentic loyalty to the common good utilize factional methods which only further contribute to the corrupted political practices. In spite of relatively sincere beginnings, a public interest faction maximizes the long-term dangers of the faction by quite unconsciously asserting its own prerogatives while the members still believe they are serving the community.

The crucial factor in the unstable competition among factions is the possession of the government. The government can be regarded as a complex of symbols, office, institutions, laws and personnel to make, administer and enforce concrete policies. This complex confers the mantle of legitimate concern for the common good and can still command some residual loyalties among all citizens. Since it is systematically and constantly financed, the government exists independently as a set of resources which can be "captured" by any faction.

In theory, the government is the institution open to maximum participation by all citizens. It passes and administers laws which guarantee the economic and moral integrity of *all* citizens. By the mobilization of overarching loyalties, it provides the means of reconciling conflict, maximizing communal cooperation, and maintaining common defense. Politics ought to be the arena in which the greatest amount of human virtue and care is exercised. In reality, the government becomes a factional tool used by individual factions to protect and aggrandize themselves while limiting the power of other factions. The government might begin to act as a faction and thus encourage even greater and easier penetration of the government by self-interested factions. Once the dynamics of competition and factionalization begin, they extend to all sections of the state. Only by aligning with or forming a faction can citizens hope to influence policy effectively. Even virtuous and loyal citizens are reduced to the expediency of so-called "public interest" factions.

The relations between quasi-autonomous factions and the government are quite complex. The dominant factions need to maintain an effective government which can resist or control the poor.[24] The government will either repress the poor or confuse them with illusions of efficacy, while the factions ensure their own power and penetrate the government by subversion of office or party. This permits the government to control the masses while neutralizing its threat to the dominant factions.

When a number of fairly coequal factions compete, government can also serve some important functions. A "balance of power" politics might emerge where the government performs a referee function. Overall, it would legitimize the power and wealth distribution, perform effective police and defense functions and prevent the factions from tearing each other apart and upsetting the balance. As a regulator of the poor or a referee, the government enforces "the rules of the game" to minimize violence and regularize relations among factions.

The moral and political relations of factions to the community are primarily those of convenience, not loyalty. Contractual arrangements and bargaining do not reflect a moral consensus. The dynamics of factions are towards dominance and control, not simple competitive co-existence. While this dominance may often be indirect, the domination of all or part of the state by a family, corporate entity or other faction is a constant tendency of the politics of faction. Even war will not dampen the competition among factions to control the state–it may make the competition worse. Thucydides' narration of the history of Cleon or Alcibiades and their factions in Athens demonstrates the type of conflict involved. In his analysis of Genoa, Machiavelli explores a totally corrupt state where a private corporation of merchants, the Bank of San Giorgio, achieved such a degree of control and discipline that it performed all the effective functions of rule; the government and the rest of the state were totally without effective control.[25]

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[23] Machiavelli (1965, *History of Florence*, Bk. 4, Ch. 27; Bk. 5, Ch. 31; Bk. 7, Ch. 1); Rousseau (1964, *Du contrat social*, Bk. 4, Ch. 1; *Discours sur l'economie*, pp. 245–47, 252–54).

[24] Aristotle (1962, Bk. 5, Ch. 6); Rousseau (1964, *Discours sur l'inégalité*, pp. 176–78).

Patterns of a Corrupt Politics

Inequality combined with the decline of civic virtue and factional competition produces several characteristic patterns of politics. These are: (1) the disintegration of effective public law and fair adjudication, (2) the decline of a meaningful political discourse, (3) the emergence of violence as the dominant substratum of legal and political relations, (4) the constant tendency towards demogoguery and class war, and (5) an increasing unlikelihood of successful reform or revolution.

1. *Law and Adjudication.* Human beings are loyal to internalized laws which they accept and which embody their beliefs and emotional commitments. When people are loyal, even to themselves, it is almost always to impersonal rules of conduct embodied in an individual's character.[26] Political laws are the medium by which politics and personal morality interrelate. They are embedded in the education, the customs, the habits, and the social pressures of daily life and form the bases of concrete moral loyalties. The legal and judicial systems give substance to the moral and economic integrity of the citizens and assure reasonably impartial treatment for all citizens.

The effectiveness of laws depends upon a complex of factors. Only when the vast majority of citizens spontaneously accept the laws even when they disagree with them, can law be a tool for community direction and reform. In a healthy state coercive enforcement is peripheral to the law. This loyalty is reinforced by constitutional limits, political participation and reliable and fair methods of adjudication and conflict resolution.

Once certain groups can unduly influence legislation, buy immunity from judgment and punishment, or use the judicial system against their opponents, citizens will lose faith in the state and join factions for their own protection.[27] The fractious populace views law as privately legislated, selectively enforced and administered on the basis of privilege, not equity. Law loses the trust which it needs for practical effectiveness, and obedience even to good laws must be forced or bribed.[28] This cynical rejection of law, except when necessary or convenient, saps the vitality of law for political direction.

Another litmus test of a corrupt polity occurs when even reform laws are futile and sometimes precipitate far more harm than good. The rich dominant factions will ignore them with virtual impunity; the administrators will sabotage them; the citizens whom they are designed to benefit will mistrust them or use them as a pretext for violence. For example, when the Gracchi attempted to resurrect the egalitarian Agrarian Laws in the late Roman Republic, the laws were unenforceable against the upper classes and the Gracchi's efforts precipitated uncontrollable violence and the Marian civil wars.[29]

While the dominant factions might use the legal system for conflict resolution among themselves, the law is unable to control most factions for the common good. This corruption of fair conflict resolution leaves violence and subversion as the methods of redress. The lack of "normal" access engenders more apathy and violence especially among minor factions and atomized masses.

2. *Political Discourse.* The transformation of law into a symbol of oppression rather than equality or equity reflects the destruction of a viable political discourse.[30] An effective poli-

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[25] Machiavelli (1965, *History of Florence*, Bk. 8, Ch. 29). The story of Cleon is narrated in Thucydides (1934), Bks. 3–5, and the story of Alcibiades in Bks. 6–7.

[26] Royce (1969, Pt. 2, Chs. 1, 5, esp. 866 ff.).

[27] Machiavelli (1965, *Discourses*, Bk. 1, Chs. 7, 8, 24; *History of Florence*, Bk. 3, Ch. 5; Bk. 4, Ch. 28).

[28] Machiavelli (1965, *Discourses*, Bk. 1, Ch. 55); Rousseau (1966, Ch. 20).

[29] Machiavelli (1965, *Discourses*, Bk. 1, Ch. 37).

[30] Wolin (1960, Ch. 1) mentions certain classical themes which identify both political philosophy and politics as human activity. Pocock (1973, Chs. 1, 2, 7) argues that these themes and countless others are embedded in languages, customs and symbols of peculiar historical eras. They carry significant power to explain, justify and persuade individuals of those eras. Pitkin (1972, Chs. 3, 5–7, 9) provides part of the epistemological bases for the linguistic aspects of the theory of political discourse. The study of the symbolic, as opposed to purely linguistic, aspects of the theory has been carried out by authors too numerous to mention; notable among them are Ernst Cassirer and George Herbert Mead. Edelman (1964) summarizes them.

tical discourse depends upon the ability of traditional political symbols and rhetoric to evoke spontaneous emotions of affirmation for the possessors of the symbols. It also assumes that the structures covered by the symbols are capable of rational evaluation and discussion to complement and deepen the emotional affirmation. In non-corrupt discourse the symbols not only evoke trust and loyalty in citizens but call forth a deep sense of responsibility from those in authority. "The office makes the man." Oaths, laws and the moral goals of the community are rationally known and emotionally compelling to both citizen and ruler.

In a healthy state the meanings of the basic political symbols are firmly established. The political battles are over the possession of those symbols and the policies which they justify, but do not involve fundamental ideological conflicts. Political activity is removed from simple coercive power relations. Reason is delimited by points of authoritative reference and a degree of peace and consent exists which enables people to persuade one another rather than resort to force. An authentic political discourse establishes a realm of ordered coherence where nonoppressive patterns of authority can exist and evolve.

This orderly realm may escape the stigma of corruption for a long time. Constant efforts will be made under its aegis to remedy injustice and inequality. All classes, particularly the upper classes, will be recalled to symbolic adherence to the common good. Reformers in Florence and the Roman republic constantly tried to use the institutions and laws to "restore" the state.[31] However, the solutions in a corrupt state are usually incomplete and cosmetic. The basic inequality will still remain and citizens will finally recognize the futility of attempts to "restore" or "renew" the commonwealth. The recognition of the hypocritical manipulations of the government by the upper classes, gives the *coup de grace* to any rational or emotional loyalties which cut across class or faction.

Thus corruption destroys the coherence of the political discourse. Exercising little emotional or rational power, symbols are used to rationalize gain. Political controversy now often focuses on the very meaning or existence of the symbols. When symbols do command the loyalty of some citizens, they confuse and oppress the people by giving them false hope.[32] Thucydides relates how oaths, promises and treaties were violated whenever "calculation" suggested to a Greek faction that it might gain some advantage from treachery. Such sophistication becomes equated with "superior intelligence." Calumny becomes a normal rhetorical tool and good advice is often ignored because of impugned motives. With no "language of persuasion," almost any incident from a slander to sexual infidelity can unleash violence in the tinder box of a corrupt state. Political rhetoric degenerates to a politics of noise.[33]

3. Violence. With the greater inequality and the decline of the legal system and political discourse, normal political relations in the state are increasingly undergirded by violence. There is no longer the requisite consensual trust or loyalty to generate sufficient patience and compromise for workable and peaceful solutions to political problems. Crime increases in all orders of society and, although unable to deal effectively with upper-class crime, the government increasingly resorts to imprisonment and repression of citizen criminals. Respect for law declines and ruling requires greater emphasis upon coercion or social bribery.[34]

While physical violence is largely confined to relations between the rulers and masses, competition among factions involves increasing levels of deception, treachery, bribery and covert violence or assassination. Balance of power politics and the hope of economic gain may often lead to somewhat stable arrangements among the dominant factions, but the alliances become more brittle and disintegrate if any of the partners see a chance of significant gain. The "rules of the game" are increasingly violated and as they decline in efficacy, non-integrative violence increases. Machiavelli recounts how in the early Roman republic violence in the state identified weaknesses and enabled the republic to reintegrate and develop enduring solutions to the problems. But as factions became more polarized, the violence led to civil war and disintegration.[35]

Political solutions rest on a foundation of predatory violence and fear, not internalized acceptance. Coalition building becomes so unstable that even long-term interest and familial ties cannot hold together factional alliances.[36] As the conflict becomes more acute, factions resort increasingly to imprisonment, exile and indirect punishments or segregation of other factions. Fair proposals for compromise are usually ignored and promises are soon broken. Fearing reprisals from the other side and having no confidence in the integrity of the government, more factions resort more quickly to violence or treachery. The moral bankruptcy of communal loyalty reaches its epitome when factions call in foreign intervention on their behalf even at the jeopardy of the state. Thucydides recounts how factional bitterness in Corcyra, Mytilene and Megara brought Athenians and Spartans into the cities and resulted in the end of effective freedom. For Thucydides one of the main causes of the extension of the Peloponnesian War was the tendency of corrupted factions to invite the dominant powers in to destroy their enemies. Machiavelli remonstrates about this same danger when he recounts the history of the Guelfs and Ghibellines in Florence and Italy.[37]

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[31] Machiavelli (1965, *Discourses,* Bk. 1, Ch. 7; Bk. 3, Ch. 1 and passim; *History of Florence,* passim).

[32] Rousseau (1964, *Discours sur les sciences,* pp. 5–26; *Discours sur l'inégalité,* pp. 176–84).

[33] Thucydides (1934, Bk. 3, Chs. 9–11; Bk. 4, Ch. 14; Bk. 6, Chs. 18, 19; Bk. 8, Ch. 25); Aristotle (1962, Bk. 5, Chs. 4, 5); Machiavelli (1965, *Discourses,* Bk. 1, Chs. 7, 8, 40–44; Bk. 3, Ch. 26; *Prince,* Chs. 15–19; *History of Florence,* Bk. 3, Ch. 5; Bk. 4, Ch. 28; Bk. 6, Ch. 23).

[34] Machiavelli (1965, *Prince,* passim; *History of Florence,* Bks. 6, 7).

[35] Machiavelli (1965, *Discourses,* Bk. 1, Chs. 4, 17).

4. *Class War and Co-optation.* The rise of a permanent wealthy class and factions with quasi-governmental independence signals the existence of a permanent class of poor people. The poor have no independent means of economic subsistence and therefore depend upon the community, corporate groups, and the rich. The inequality is so great that "two societies" might be said to coexist.[38]

There will be endemic conflict in a state where poverty breeds "the courage of necessity" and riches breed "ambition, insolence and pride."[39] This conflict and violence, however, can result in just and enduring solutions if there is sufficient trust and loyalty among all citizens. Both classes must be willing to compromise and not push the other side to violence. In early Rome, Athens and Florence, constant class war was mitigated with compromise solutions such as the Roman Tribunate.[40] But a just and reasonable politics depends upon the willingness of factions to set limits upon their demands and to honor compromises with fellow citizens. Once the broad consensus breaks, loyal care will not temper vicious self-interest; the demands will escalate and the two classes and factions will participate in increasingly intractable conflict with no possibility of just compromises. In Plato's terms, the state becomes "two cities" locked in irreconcilable war.[41]

Corruption also extends to the poor. The inherent degradation of utter dependence provides no social or economic basis for a sense of self. Human relations break down under the constant competition for scarce jobs and resources. The desperate economic plight of the poor inculcates avarice merely for survival. They slowly lose a sense of loyalty to a community which cannot provide dignity or freedom for them. Laws which can be manipulated at will by the wealthy lose their spontaneous acceptance. Their work relations, families, religion and even friendly associations break down and leave the poor distrustful, envious, competitive and cynical. They become easy prey for the co-optative enticements of money or volunteer as recruits for mercenary or professional armies. In the army, at least, they gain the status and money denied them as citizens.

The poor are usually characterized by political inertia; they may periodically rebel, but without leaders and organization they fail. Although atomized, the corrupt state is vulnerable to damagogues such as Cleon, Marius or Alcibiades who can lead popular uprisings and expropriation attempts.[42] The fragmentation and ineffectiveness of the under class is accentuated by a number of political patterns: the elites recruit mercenaries to quell violence; a dole is established to take the edge off of desperate poverty; and finally the political myths of a united citizenry with equality before the law are perpetuated by external wars and periodic, highly publicized legal successes.

The emphasis upon the politics of factions and class is theoretically consistent. Most effective factions are products of the upper classes. The maintenance of a power base and the capacity to suborn government require either wealth and organization or the internal cohesion of the kind found in an aristocratic family of the ruling class. The corrupted poor are notoriously deficient in all of the above. Factional competition is confined to the dominant classes and their allies with two great exceptions.

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[36] Machiavelli (1965, *History of Florence*, Preface; Bk. 4, Ch. 2; Bk. 6, Ch. 9; Bk. 7, Ch. 1).

[37] Thucydides (1934, Bk. 1, Chs. 4, 5; Bk. 2, Ch. 6; Bk. 3, Chs. 9–11; Bk. 4, Chs. 13, 14); Machiavelli (1965, *History of Florence*, Bk. 1, Ch. 4; *Discourses*, Bk. 2, Ch. 25; *Prince*, Ch. 11).

[38] Machiavelli (1965, *Discourses*, Bk. 1, Ch. 4); Rousseau (1964, *Du contrat social*, Bk. 4, Ch. 2).

[39] Thucydides (1934, Bk. 3, Ch. 9); Plato (1957, 421d–423b).

[40] Aristotle (1962, Bk. 5, Chs. 8, 9; Bk. 6, Ch. 5); Machiavelli (1965, *Discourses*, Bk. 1, Chs. 4, 17; *History of Florence*, Preface); Rousseau (1964, *Du contrat social*, Bk. 3, Ch. 9).

[41] Plato (1957, 423a).

[42] Thucydides (1934, Bk. 2, Ch. 7; Bk. 5, Chs. 14–17; Bk. 8, Chs. 25, 26); Aristotle (1962, Bk. 5, Chs. 4–6); Machiavelli (1965, *Discourses*, Bk. 1, Chs. 40, 57; *History of Florence*, Bk. 3, Chs. 13, 15–19); Plato, 565a–580e).

In the first, a member of the elite or one of the dominant factions might try to provide the poor with leadership and mobilize them as a power base. The Duke of Athens in Florence and various Athenian aristocrats resorted to this policy with varying degrees of success.[43] A leader might also gain control of the army, which is largely recruited from the poor, and use it to gain power. When the poor are armed and effectively led, class war erupts. The danger is so real that the upper classes in a corrupt polity often fear to arm their lower classes except in last-ditch defense.[44]

The second exception might be called the politics of liberation and co-optation. The seething dissatisfaction of the oppressed generates factions of a different order. These are liberation factions which have the dual goals of freeing both their own members and the entire society from inequality and injustice.

The initial mobilization usually depends upon an attack upon the regime and its symbolic panoply. The liberation attack simultaneously weakens general respect for the communal values and institutions and seeks to either "renew" or "overthrow" the state, often in the name of the very values which it attacks. Most revolutionary attempts, unless part of a general class war, will probably be repressed. In a less ambitious strategy, however, the factions might gradually win access and power and bring about some concrete changes.

The claim to serve all citizens coupled with its factional nature creates serious weakness in the liberation strategy. Success suddenly makes the maintenance of the faction's power critical. Over the long run the insurgent faction ends up imitating the dominant factions and often supports the power distribution in order to preserve its own ability to influence policy. The whole policy is assisted by the faction's ambivalent disrespect for the goals and institutions which it both attacks and exploits. This encourages its use of informal power politics rather than open participatory procedures. Ironically the faction's initial polemics often make it more difficult to resuscitate active community commitment, and as long as they accomplish something under the "reformed" system, they often do not try. One of the most common examples of this is the fate of workers' organizations. Machiavelli describes how the Florentine guilds were initially designed to gain economic and political power for workers. However, they became stratified into two different levels and one level became a part of the ruling elite while many of their own members and nonorganized workers were squeezed out.[45]

5. *The Unlikelihood of Reform and Revolution.* Once corruption becomes widespread, there is very little possibility of successful, significant reform of injustice in the state—even a successful violent revolution becomes almost an impossibility. Since the theory emphasizes the moral as well as the economic and institutional requirements of a just state, any significant legal, political or even economic changes will be quite irrelevant since the citizens will either frustrate the goals or use the new arrangements for their own radically self-interested benefit. Without the consensual loyalty and trust of the citizenry, the new reforms will simply be shams to rationalize the continuation of corrupt practices. Citing the failure of Solon's laws at Athens and the policy of equal division of land at Leucas and elsewhere, Aristotle argues that the radical redistribution of wealth will accomplish little unless the education and mores also changes. For Machiavelli, the corruption of the mores of the citizens in Florence and Rome was the final limit on all government reform.[46] This pessimism accounts for a paradox in thinkers like Plato and Rousseau who provide deep and ruthless indictments of their society but whose practical politics are fairly conservative and only ameliorative.

Revolutionary attempts at reform usually engender far more harm and tyranny than good. Any revolution is dangerous and violent and has tyrannical tendencies. When the people do not have the habits, customs and willingness to compromise and make the sacrifices for the revolutionary institutions, then the revolution usually requires a maximum of long-term violence and elite leadership to gain adherence to "reforms."[47]

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[43] Thucydides (1934, Bk. 2, Ch. 7; Bk. 8, Chs. 24–26); Machiavelli (1965, *History of Florence,* Bk. 2, Chs. 34–36).

[44] Thucydides (1934, Bk. 2, Ch. 8; Bk. 3, Ch. 9; Bk. 8, Chs. 24–26); Plato (1957, 551d–552a); Aristotle (1962, Bk. 5, Ch. 6; Bk. 6, Ch. 7). To overcome this problem the Spartan elite developed a stratagem to identify the 200 best helots and then arranged for their destruction. Having eliminated all the potential leaders among the helots, the Spartans could then arm them for defense (Thucydides, 1934, Bk. 5, Ch. 14).

[45] Machiavelli (1965, *History of Florence,* Bk. 3, Chs. 11, 12).

[46] Aristotle (1962, Bk. 2, Chs. 6, 7; Bk. 5); Machiavelli (1965, *Discourses,* Bk. 1, Chs. 17, 18; *History of Florence,* Bk. 3, Ch. 5; Bk. 4, Ch. 1); Rousseau (1964, *Discours sur l'inégalité,* pp. 191–93; *Du contrat social,* Bk. 2, Ch. 8; *Discours sur l'economie,* pp. 252–53).

There is, however, a very limited possibility of a great and revolutionary "renewal" of the country. The praxis of a violent revolution, a great religious crusade or awakening, a desperate war, or a combination of any of these are all historically verified manners of regenerating communal loyalty and concerted action to overcome class and factional barriers. Machiavelli cannot help but remark that Florence is usually most harmonious when it is engaged in war.

None of these approaches is particularly recommended. Violent revolutions rarely happen and even more rarely succeed. Religious revolutions, while more likely to succeed, are even more quickly corrupted and destroyed than secular revolutions. Wars may temporarily reintegrate the society, but the class and factional competition is only repressed. As the war goes on and the poor bear a disproportionate burden of its cost, foreign intervention becomes more likely or a dominant military leader may take over. The war will wreck the state. Only consistent good leadership, real participation and abolition of massive inequalities will "restore" the state to its original principles of justice, fraternity and equality.[48]

Education: Formal, Family, Religion and Militia

Inequality dominates the causes of systematic corruption, but human nature must also be addressed. Education and socialization must inculcate disciplined commitment to other citizens and loyalty to the commonweal.[49] Customs, habits and mores can sometimes be strong enough to sustain institutional integrity and loyalty among citizens even after great inequality exists. Education and socialization, however, fight a rear-guard action. Neither equality without education nor education without equality can sustain a just, stable and equal state. Corruption spreads beyond the political realm and cripples the structures which generate reasonably disinterested loyalty and civic virtue. As relations become instrumentalized under the pressure of inequality, citizens lose the capacity for piety, dutifulness and affectionate loyalty. Four vital areas of political socialization are undermined: formal education, the family, organized religion and mutual self-defense.

The society's civic educational system is corrupted by several onslaughts. As the corruption of values in government and the wider society becomes more apparent, it becomes harder to find teachers who can seriously teach these values. Teaching, itself, becomes an undervalued occupation in a world of great economic and social disparities, and fewer talented people enter it. Additionally the teachers and schools come under constant attack from various factions for teaching a set of values which might lead a student to question a particular faction's place in society or damage a faction's future recruitment. The schools also confront students and parents who see that the "older" concern with rational and humane mores and loyalty are counterproductive in a Prophecy and regeneration remain. The resurrection of Florence under its unarmed prophet, Savonarola, and Geneva's transformation by its armed prophet, Calvin, were classic examples of religion's "restorative" powers.[54]

The increasing dissolution of the citizens' bonds of loyalty ends the state's ability to generate its own militia. In a just and stable state a voluntary citizen army served three purposes. First, it was a counterweight to the rich and powerful. As long as the citizens controlled the main source of legitimate coercion and defense, the loyalty of the elites was reinforced by fear of arms. Second, loyal and committed citizens made better and less ambitious soldiers. Third, a participatory militia world of atomized selfishness and factional competition. The schools are slowly transformed into nothing more than occupational training for the factions and become devoid of any independent values linked to loyalty to the common good and other citizens.

The incapacity for loyalty also wrecks the social stability of the family. The loyalty of husband and wife lasts only as long as it is convenient; adultery and divorce become normal and justifiable whenever duties of fidelity interfere with immediate pleasures. As the parents liberate themselves, the children are neglected or shunted off because they seem unrewarding.

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[47] Machiavelli (1965, *Discourses*, Bk. 1, Chs. 16–18); Rousseau (1964, *Du contrat social*, Bk. 2, Chs. 8–10).

[48] Thucydides (1934, Bk. 2, Ch. 7; Bk. 6, Chs. 18, 19; Bk. 8, Chs. 24–26); Aristotle (1962, Bk. 5, Ch. 7); Machiavelli (1965, *History of Florence*, Preface; Bk. 2, Ch. 27; Bk. 3, Ch. 11; Bk. 4, Ch. 28; *Discourses*, Bk. 1, Ch. 25; Bk. 3, Ch. 1); Rousseau (1964, *Du contrat social*, Bk. 2, Ch. 8).

[49] Plato (1957, 386a–416c; 423e–424c); Aristotle (1962, Bk. 2, Chs. 7, 8; Bks. 7, 8); Rousseau (1964, *Du contrat social*, Bk. 2, Chs. 6, 12; *Discours sur l'economie*, pp. 260–61).

The lack of loyalty and care in the family destroys the family as a socializing agent. In families citizens acquire basic moral beliefs and learn rudimentary forms of justice, cooperation and affirmation of authority.[50] As parents betray one another and lose confidence in their authority, children learn to ignore parental authority and pursue their own interests. Individuals learn to perceive all law and morality as oppression.[51] If children have no respect for rules given by parents, they will never accept laws which impinge upon them for the benefit of others.

The corruption of organized religion destroys another voluntary organization which sustains moral commitments to others.[52] The change is not so much one of religiosity as of piety. The moral claims of religion to limit avarice or encourage charity lose their force. Fear of God wanes and the self-sacrifice of piety is outweighed by love of gain.

The decay of religion occurs on two levels. First, citizens slowly leave the churches or transform them into purely social or private activities. Second, the church itself becomes a faction. To maintain its institutional power it might ally itself with the elite and then act as an agent of control rather than one of grace and worship. The constant vacillation of the Delphic oracle among the various Greek factions reflects such bankruptcy. The religion might also follow the strategy of the Roman Catholic Church of Machiavelli's or Rousseau's time and use its spiritual authority to gain riches, land and power for itself while sacrificing the moral integrity of its leaders and the spiritual welfare of its members.[53]

Religion's inherently mysterious and evocative relation with people gives it the constant potential to renew the moral life of the community. Its clergy can be corrupted, its membership thinned, but the possibility of was a great equalizer. It pulled all classes of society together and made it more democratic in its values and reinforced the loyalty of citizens for one another.[55]

In an unequal and corrupted state the bulk of the citizenry have little reason to defend a state which gives them so little. The elites care too much for themselves and possess their own means of protection. They also fear to see the poorer citizens armed. The state is reduced to expedients for defense: payoffs to enemies, mercenary soldiers, wars by proxy and a professional army. The bribery scheme works in the short run, but it is too dangerous in the long run and often generates internal unrest because of the humiliation and cost involved.[56] Mercenaries, like Francisco Sforza, The Duke of Milan, are inefficient, expensive, often disloyal and liable to turn on the country and conquer it.[57] Proxy wars, as the Athenians discovered in trying to rule their empire indirectly, are extremely costly and they usually involve unreliable allies and pull the state into increasingly larger and costlier intervention.[58] The last solution, the professional army, is much more militarily efficacious, but it poses a great threat to internal freedom. The army is loyal to those who pay it and can easily become an adjunct to the ruling classes. The maintenance of a standing army involves larger budgets and creates many opportunities for corrupt alliances between the military and various economic factions which supply it. Finally, if the army should develop its own inner cohesion, the army can become the most powerful faction in the state. The state can either buy it off with great sums of money or the military faction may sell itself to a political entrepreneur or simply take over the government.[59]

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[50] Machiavelli (1965, *Discourses*, Bk. 1, Chs. 11–15; Bk. 2, Ch. 2; Bk. 3, Ch. 33); Rousseau (1964, *Discours sur l'economie*, pp. 261–62).

[51] Plato (1957, 553a–553e; 562e–565e).

[52] Machiavelli (1965, *Discourses*, Bk. 1, Chs. 11–15; Bk. 2, Ch. 2; Bk. 3, Ch. 33); Rousseau (1964, *Du contrat social*, Bk. 2, Ch. 7; Bk. 4, Ch. 8).

[53] Thucydides (1934, Bk. 1, Chs. 5, 6); Machiavelli (1965, *Discourses*, Bk. 1, Ch. 12; *History of Florence*, Bk. 8, Ch. 17; *Prince*, Chs. 7, 11, 12); Rousseau (1964, *Du contrat social*, Bk. 4, Ch. 8).

[54] Rousseau (1964, *Du contrat social*, Bk. 2, Ch. 7); Machiavelli (1965, *Prince*, Ch. 6; *Discourses*, Bk. 2, Ch. 16; Bk. 3, Chs. 1, 24; Letter 3, Vol. 2, pp. 886–89).

[55] Machiavelli (1965, *Discourses*, Bk. 2, Ch. 10; *Art of War*, Preface; Bk. 1); Rousseau (1964, *Considérations sur le Pologne*, pp. 1012–20).

[56] Machiavelli (1965, *Discourses*, Bk. 2, Chs. 10, 30).

[57] Machiavelli (1965, *Prince*, Chs. 12, 13; *History of Florence*, Bks. 1–6, passim, esp. Bk. 1, Ch. 39; Bk. 4, Ch. 24; Bk. 5, Ch. 34; Bk. 6, Chs. 1, 20).

[58] Thucydides (1934, Bks. 3–8, passim, esp. Bk. 3, Chs. 10, 11; Bk. 5, Ch. 16; Bk. 7, Ch. 21; Bk. 8, Chs. 24–25; Bk. 1, Ch. 4).

[59] Machiavelli (1965, *Art of War*, pp. 566–76; *Prince*, Chs. 6, 12; *Discourses*, Bk. 2, Ch. 12); Rousseau (1964, *Discours sur l'economie*, p. 269).

Conclusion

The corruption of a state encompasses the changes in the aggregate social and political relations which in turn reflect the changes in the basic moral constitution of the citizens. While this theory provides a model of the relations among inequality, mores, factions and politics; it makes no pretense of neutrality. It explicates the decay of a socio-political unit into a world where right, fulfillment and happiness are systematically unattainable in normal relations among citizens. This recognition of the sadness and evil of the phenomenon makes most of the analysis a prelude to prescriptions. The theory is militantly non-utopian and recognizes that no social or political system will ever "transform" human beings into spontaneously altruistic and caring individuals with sufficient strength to transcend the temptations of selfishness and arrogance. It also manifests an abiding pessimism about the success of significant reforms once corruption becomes widespread; it does, however, offer a number of positive prescriptions for struggling states.

First, corruption is a part of the human condition, and practical honest politics requires structures designed to limit, discourage, and channel these tendencies. Day-to-day politics must be conditioned by a concrete evaluation of the moral beliefs of citizens and their willingness to obey laws. There are times when even good and well-intentioned laws would create nothing but social unrest and actually undermine the goal for which they were designed.

Second, any viable polity must concentrate upon education to inculcate loyalty towards fellow citizens and create an initial willingness to sacrifice self-interest to the common welfare. The need for universal political education is the most important aspect of politics. Primary institutions which lay the foundations of a just and stable authority must have strong legal and structural supports.

Third, political participation continues the process of political education and contributes to the stability of the regime. Massive substantive participation by all citizens in public office and public service can offset the unequal classes and factions. It also democratizes and reinforces the loyalty of all citizens. This openness and participation also encourages the maximum use of the political processes to accommodate conflict and violence. In the long run, open participation through limited terms in office, widespread elections, and rotation of all bureaucratic offices will prevent governmental agencies from turning into isolated non-responsible factions.

Finally, it is absolutely necessary that severe limits be placed upon great accumulations of wealth and hereditary privilege. The entire dialectic of injustice and corruption begins with such inequality. A healthy polity must prevent any effective derogation of its power to private governments and destroy any factions which gain enough power to consistently subvert the law. The theory insists upon the absolute danger of a permanently impoverished class. Politics must insure the economic integrity of all citizens in terms of basic needs and work or run the risk of complete corruption and class war.

The corruption of states and the corruption of people proceed hand-in-hand. It has been the dream of diverse ideologies that one problem could be solved without the other—my analysis suggests otherwise. To take corruption seriously is to take civic virtue seriously; to take civic virtue seriously is to demand not just moral education but to demand substantive participation, and economic and political equality.

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Political Corruption in America: A Search for Definitions and a Theory, or

If Political Corruption Is in the Mainstream of American Politics Why Is It Not in the Mainstream of American Politics Research?*

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Lack of a clear definition of political corruption has limited its systematic study by analysts of American politics. This article offers a conceptual framework with which to view corruption. A corrupt act is categorized by its four components: the donor, the favor, the public official and the payoff. For each component, propositions about perceived corrupt and noncorrupt elements can be formulated and tested. The usefulness of this scheme in analyzing attitudes about corruption is demonstrated with data from state legislators. Finally, the article suggests some future research possibilities using this scheme to compare elites and public or other groupings in the political system.

Though corruption has been an everpresent part of American political life (cf. Tocqueville, 1861), analysts of American politics have not studied it systematically (for exceptions, see Greenstein, 1964; Gardiner, 1970; Wolfinger, 1972). This paper offers a conceptual scheme which circumvents definitional problems that have posed such a roadblock to the systematic study of corruption. We also provide some attitudinal data about corrupt acts gathered from state senators across the U.S. and show how our scheme contributes to the analysis of this data. Finally, some reasonable future research directions for the study of corruption are briefly discussed.

Definitions of Political Corruption

The attention devoted to a serious examination of corruption in America occurs largely at those times when particularly venal acts have been exposed. Thus, the post-Watergate period has brought with it a renewed interest in the study of political corruption, especially among political scientists (cf. Rundquist et al., 1977; Scoble, 1973; Gardiner 1970, Berg et al., 1976). In all of these studies it becomes immediately apparent that no matter what aspect of American politics is examined, the systematic study of corruption is hampered by the lack of an adequate definition. What may be "corrupt" to one citizen, scholar, or public official is "just politics" to another, or "indiscretion" to a third. Several definitions of political corruption have been proposed and generally can be classified according to three criteria: definitions based on legality, definitions based on the public interest, and definitions based on public opinion (Scott, 1972).

The definition of political corruption based on legalistic criteria assumes that political behavior is corrupt when it violates some formal standard or rule of behavior set down by a political system for its public officials. Perhaps the clearest statement of this definition has been given by J. S. Nye when he stated that a political act is corrupt when it "deviates from the formal duties of a public role (elective or appointive) because of private-regarding (personal, close family, private clique) wealth or status gains: or violates rules against the exercise of certain types of private-regarding influence" (Nye, 1967, p. 416). While such a definition of corruption is useful to the researcher in that it is generally clear-cut and can be operationalized, when the behavior in question allegedly deviates from a legal norm or standard which is not tied to a specific statute or court ruling, this definition of political corruption becomes less useful as the formal duties of office or the appropriate rules of influence become ambiguous. Moreover, this definition suffers from being simultaneously too narrow and too broad in scope; all illegal acts are not necessarily corrupt and all corrupt acts are not necessarily illegal.[1]

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Definitions of political corruption based on notions of the public or common interest significantly broaden the range of behavior one might investigate. Consider the definition proposed by Arnold Rogow and Harold Lasswell: "A corrupt act violates responsibility toward at least one system of public or civic order and is in fact incompatible with (destructive of) any such system" (Rogow and Lasswell, 1966, pp. 132–33). While this definition focuses our attention on any act or set of acts which threaten to destroy a political system, the researcher has the responsibility of determining what the public or common interest is before assessing whether a particular act is corrupt. The possibility exists that a behavior may be proscribed by law as corrupt but be beneficial for the common good, such as "fixing" the papers of an illegal alien who contributed his labor and skills to a rapidly expanding economy. Furthermore, this definition enables a politician to justify almost any act by claiming that it is in the public interest.

A third approach to the definitional problem suggests that a political act is corrupt when the weight of public opinion determines it so (see, for example, Rundquist and Hansen, 1976). This conception of political corruption harbors the same limitations as the public interest focus. Studies of public opinion have revealed that on many issues public sentiments are either ambiguous (significant portions of the public hold no opinion or hold those of low intensity) or are divided in their opinions. Additionally, a definition of corruption based on public opinion must consider the differences which may exist between the public and political elites in their assessment of appropriate standards of public conduct.

This approach to political corruption is probably best illustrated in the work of Arnold J. Heidenheimer (1970). In his view the corruptness of political acts is determined by the interaction between the judgment of a particular act by the public and by political elites or public officials. According to this scheme, behavior is judged particularly heinous or corrupt if *both* public officials and the public judge it corrupt *and* both wish it restricted. This type of behavior is referred to as "black" corruption. An act such as "a public official involved in heroin trafficking" would most likely fit this category in that both groups find the act reprehensible and would demand punishment for the guilty public official. At the other end of the corruption spectrum might be categorized political acts judged corrupt by both public officials and the people, but which neither feel are severe enough to warrant sanction. Quite possibly such acts of "white" or petty corruption as a city council member fixing a parking ticket for a constituent fall into this category. Between these two extremes of corruption acts lie the forms of behavior which are the most difficult to define and detect, and consequently are potentially most destructive to a political system organized along democratic principles. Heidenheimer refers to these political acts as "gray corruption" when either public officials or the people want to see an action punished, while the other group does not, or it may well be that one group is intense about the issue and the other ambivalent or unconcerned.[2] Heidenheimer's work in this area, therefore, points to the existence of a scale or dimension of corruption that can be used to classify political behaviors according to their degree of corruptness from "black" to "gray" to "white." It does not, however, account very well for those acts seen as corrupt by only one group, nor does it seek to explain *why* some groups may see an act as corrupt but other groups see it as less corrupt. Although this conception of political corruption is based on the criterion of opinion (both public and elite), the assessment of a specific political act may rest on violation of a legal norm or a threat to the public interest. In other words, definitions of corruption are not mutually exclusive: elements of the public interest and public opinion criteria are embedded in legal norms which sanction certain political behaviors as corrupt.

Although the Heidenheimer scheme enables us to classify politically corrupt acts in a general way, a more detailed scheme seems required if we are to classify adequately the many variations of corrupt acts, and if we are to develop an explanation of why some acts are judged corrupt and others not. We propose to analyze potentially corrupt acts according to the component elements apparently involved with every political act or exchange. We believe this process can meaningfully be partitioned into the "public official" involved, the actual "favor" provided by the public official, the "payoff" gained by the public official, and the "donor" of the payoff and/or "recipient" of the "favor" act. Although at this early stage in the development of a theory of political corruption it would be too much to claim to be able to specify the exact nature of each of these components and its relation to the others, we do believe that examining acts of political corruption in this manner might hold the key to a better understanding of why public officials perceive some acts as corrupt and others as "just politics," and why public officials and the public may differ in their assessment.

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[1] Berg, Hahn, and Schmidhauser (1976, p. 170) discuss seminars being conducted for large campaign donors on how to use the loopholes in the new campaign fund laws.

[2] Harry Scoble (1973) refers to the situation (gray corruption) where public officials tolerate a corrupt act or practice and citizens are unaware or ignorant of the act but would condemn it if they knew about it as "systemic corruption." Moreover, this view of political corruption is useful and important in that it leads to an emphasis on the basic defects and weaknesses in the political system which may be responsible for corruption (see Berg, Hahn, and Schmidhauser, 1976). Therefore, rather than attributing such phenomena as "Watergate" to the weaknesses and foibles of individual political actors, the "systemic view" of corruption would lead to a search for the sources of "Watergate" in the defects of the political process itself.

When discussing the subdimensions of each component of a potentially corrupt act, we will be stating some propositions about what acts will be seen as corrupt. Those propositions will be discussed in some detail later. When examining a "public official" involved in an alleged act of corruption, we are particularly interested in whether the act was entered into in the performance of the official's *political* duties. Presumably, an act which is considered malfeasance, misfeasance, or nonfeasance of public duty is *more* corrupt than a behavior engaged in outside of one's official political role. In other words, misusing one's political office for private gain is more objectionable than engaging in questionable behavior outside of one's official duties.[3]

A second characteristic of the public official that seems to determine whether behavior is to be judged corrupt is the political nature of the public official's role. If a public official is in a judicial or other nonpolitical post, certain acts are more likely to be seen as corrupt than if the public official holds a political post. For example, judges have traditionally been held to higher standards with regard to conflict-of-interest situations than legislators. With the new congressional actions to regulate conflict of interest, it is possible that this situation may be changing at the national level, however.

The second component of a politically corrupt act is the "donor" of the payoff or "recipient" of the political favor. It is most

Table 1. Components of a Potentially Corrupt Act and Some Salient Characteristics

Component	More Corrupt	Less Corrupt
Public Official		
Type of Position	"Nonpolitical," i.e., the official is a judge	Political
Role when Act Performed	Public Role, i.e., act is done as part of official's public duties	Private Role, i.e., act is performed by public official acting as a private citizen
Donor's Relation to Public Official	Nonconstituent pays official Public official pays self	Constituent pays official
Favor Rendered by Public Official		
Type of Benefit	Private	Public
Type of Recipient	Nonconstituent	Constituent
Nature of Act Providing Favor	Nonroutine, i.e., official departs from normal routine to provide favor	Routine, i.e., favor is performed as routine part of job
Payoff		
Size	Large	Small
Time when Benefits Accrue to Donor	Short-range benefit	Long-range benefit
Substance	Specific	General
Relation to Campaign	Noncampaign	Campaign

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[3] The recent example of Congressman Allan Howe's (D-Utah) alleged solicitation of a prostitute might be a good example. He committed an illegal act, but it would hardly be called the misuse of public office. This is not to say that the voters in his district judged him any less harshly. The other recent "congressional sex scandal," that of Congressman Wayne Hayes, who allegedly put Elizabeth Ray on the payroll only because she was his mistress, more rightly fits into the misuse of public office category, and thus would be judged more politically corrupt.

important to determine if the "donor" is a *constituent,* broadly defined, or a nonconstituent. If the donor is a constituent, then a "favor" given is more likely to be perceived as *less* corrupt than if the favor is rendered to a nonconstituent. The reason is obvious: acts which are performed under the rubric of "constituency service," no matter how questionable, have a certain legitimacy. Of two similar, potentially corrupt acts, the odds are that the constituency-related act will be judged less corrupt than the service rendered to the nonconstituent. Beard and Horn (1975) offer two examples supporting this proposition. In one case, members of Congress were asked about the legitimacy of putting favorable articles about campaign donors in the *Congressional Record,* as a "thank you" to donors. This was seen as more legitimate if the donor were a constituent rather than a firm outside the district. A second example involved accepting rides to and from one's congressional district on a national firm's private plane. If the firm had ties to the constituency, accepting such favors was seen as more legitimate than if the firm had no such ties. Probably the most corrupt situation is when the donor is the public official himself. If the public official can directly enrich himself by tapping the public till, he himself is the donor. Examples of this include padding the expense account, using public funds for personal travel, using money allocated for office expenses for personal activities, and so on.

Another characteristic to be considered is whether the "donor" of the payoff (recipient of the "favor") is more than one private individual or firm. We argue that the single donor will be perceived as making an action more corrupt than if the donor is a large group of individuals or firms.

A third component to consider is the "favor" provided by the public official. We can surmise that the corruptness of the favor will vary in some ways as does the nature of the donor. Private favors and nonconstituency favors will be seen as more corrupt than those with large public benefit or those done for a constituent.[4] Finally, if the favor is done in routine performance of duty rather than extraordinary activity, it is less likely to be seen as corrupt. For example, a member of Congress who makes a routine phone call to a federal agency to check on a federal contract of a firm whose officers supported the Congress member in an election campaign is less likely to be viewed as performing a corrupt act than one who makes threats or acts in a way that the federal bureaucrat perceives as nonroutine.

The fourth component of a potentially corrupt act, the "payoff" given to the public official, is possibly the most important determinant of its perceived corruptness. The obvious fact to consider is, of course, the size of the payoff. It should come as no surprise that the larger the payoff, the greater the perceived degree of corruptness in the act, although as we shall see later on, just what is "large" can be difficult to agree upon. The payoff can also vary according to the long- versus short-range nature of the benefit to the public official. We would assume that a short-range benefit would be perceived as more corrupt than a payoff that will yield benefits at a much later date, because the long-range payoff is separated in time from the favor done. Consider, for instance, an official of a regulatory agency who takes a lucrative position in a regulated corporation after leaving government service. Presumably the official looked with some benevolence on the activities of the corporation while in public office; if that official had been given an immediate tangible payoff, for example a gift of money, we think most people would charge corruption. But the later payoff probably is seen by many as legitimate.[5]

In a similar vein, the payoff can be distinguished as to whether it is specific or general. If a payoff takes specific form, such as money or a service rendered, it will likely be perceived as more corrupt than a general payoff, such as future electoral support or good will. Although this is a rather special case of payoff, it is also important to determine if a gift is related to a political campaign. Payoffs in the form of campaign contributions have a legitimacy not rendered to other forms of material payoffs. A $1,000 cash donation to a campaign is perfectly acceptable, while an equal amount offered as a personal gift to a public official subjects the donor and official to possible legal penalties.

In Table 1, we have summarized the basic components of a politically corrupt act, and indicated the dimensions in each component which can vary according to perceived corruptness. In outlining these components and their salient characteristics, we have articulated a large number of testable propositions about the conditions under which an act can and will be viewed as corrupt. We have only outlined propositions at the first level, i.e., dealing with each subdimension singly, and have not defined propositions dealing with more than one subdivision at a time. Certainly an act having most or all of the characteristics listed on the left side is considered more corrupt than one having only characteristics on the right. Which components are most important in determining corruptness is a researchable question not answered here. Yet taking a broader view, our rudimentary scheme offers a conceptual framework for analyzing and comparing potentially corrupt political acts. This framework allows us to circumvent the definitional problems surrounding the meaning of "corruption."

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[4] Even though the dimensions of "favor" are similar to those outlined for the "donor," there are some differences. For example, it is conceivable that a private donor might pay for a favor having public benefit.

[5] President Carter, however, has extracted pledges from his high-level appointees that they will not take jobs with firms doing business with or regulated by their agency for a period of two years.

The Study Design

Our study was designed specifically to ascertain information on attitudes about corruption held by a large group of public officials. We mailed questionnaires to all 978 state senators in 24 states.[6] After three mailings, 441 senators had responded with completed questionnaires.[7] Response rate by state ranged from only 21 percent in California to over 78 percent from North Dakota. Generally, response rate was slightly higher in the rural states than in the industrialized ones.[8]

The major focus of the questionnaire was a series of items concerning ten actions by public officials that might or might not be considered corrupt. As sometimes happens, we have since found problems and lack of clarity in some of the items. These problems will be discussed later. The items are as follows (in the order listed on the questionnaire):

1. A presidential candidate who promises an ambassadorship in exchange for campaign contributions (AMBASSADOR);
2. A member of Congress using seniority to obtain a weapons contract for a firm in his or her district (WEAPONS).
3. A public official using public funds for personal travel (TRAVEL);
4. A secretary of defense who owns $50,000 in stock in a company with which the Defense Department has a million-dollar contract (DEFENSE STOCK);
5. A public official using influence to get a friend or relative admitted to law school (LAW SCHOOL);
6. The driveway of the mayor's home being paved by the city crew (DRIVEWAY);
7. A state assembly member while chairperson of the public roads committee authorizing the purchase of land s/he had recently acquired (LAND SALE);
8. A judge with $50,000 worth of stock in a corporation hearing a case concerning that firm (JUDGE);
9. A legislator accepting a large campaign contribution in return for voting "the right way" on a legislative bill (RIGHT WAY);
10. A member of Congress who holds a large amount of stock (about $50,000 worth) in Standard Oil of New Jersey working to maintain the oil depletion allowance (OIL).

We omitted items on which we felt there would be near total consensus, either because they were so serious or because they were so trivial: at one extreme, for example, a public official engaging in heroin traffic, and at the other, a policeman taking a free cup of coffee from a local cafe. We asked several questions about each item, with five degrees of response. In this article we will examine responses to the question as to whether the act was believed to be corrupt or not.

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[6] All of our mailings, sent in October, December and January, 1975–76 were sent to the senators' home addresses, in order to minimize the possibility that the senator would delegate filling out the questionnaire to a staff member or legislative intern. The states surveyed were ALABAMA, California, Connecticut, Florida, Georgia, Illinois, IOWA, Kansas, Kentucky, Maine, Massachusetts, MICHIGAN, Minnesota, MISSOURI, Nevada, New Jersey, New York, NORTH DAKOTA, Oregon, PENNSYLVANIA, TEXAS, Utah, West Virginia, and Wyoming. These states were picked randomly, with a few substitutions to assure geographic dispersion (after our initial random selection, we had eight southern states but no states from the industrial Middle West). Only some of the nonrespondents, those from the states listed in capital letters, were sent a third-wave questionnaire.

[7] For a discussion of the rationale behind the shorter, third-wave questionnaire, see Welch and Peters (1977a).

[8] In addition to California, only New York (25 percent) had a response rate of less than 30 percent. Three more had response rates less than 40 percent: Massachusetts (30 percent), Illinois (32 percent), and Massachusetts (32 percent). On the other hand, eight states had a response rate of more than 50 percent: North Dakota (78 percent), Utah (62 percent), Iowa (62 percent), Alabama (60 percent), Maine (58 percent), Kansas (53 percent), Wyoming (53 percent), and Oregon (53 percent).

Findings

We do not have enough items to explore systematically a number of propositions that could be generated by our work, but we will use these data in ways that indicate the usefulness of our scheme in analyzing corruption. We first ranked the items according to the proportion agreeing that the act was corrupt or very corrupt (Table 3). In Table 2 we have sketched the most salient characteristics of each act, according to our fourfold scheme; we have also outlined the characteristics of a hypothetical act in which a member of Congress in a district with a large percent of minority populations votes in favor of the civil rights act (CIVIL RIGHTS). In return, members of civil rights groups in the district support the member of Congress in a reelection bid. We judge that few would see anything corrupt in this sequence of events.

The first conclusion from Table 3 is that the acts considered corrupt by most people have many characteristics starred (where starred characteristics indicate more corrupt features, i.e., features on the left-hand side of Table 1). Clusters of stars are particularly apparent in the "payoff" boxes. Acts considered by few to be corrupt have few stars; CIVIL RIGHTS has only one. Those acts whose corruptness is most disputed have an intermediate number of characteristics starred. We can also immediately see that any one attribute, in isolation, probably does not determine corruptness. For example, an act performed in one's public role may lead to corruption, but simply knowing that an official performed an act as part of a public role tells us nothing by itself. Unfortunately not enough information was given to classify fully some of the acts on our four components. Greater specificity may have changed the public officials' responses, and would also have made the pattern of classification clearer.

Four acts were perceived as corrupt by over 90 percent of our sample: RIGHT WAY DRIVEWAY, TRAVEL, and LAND SALE. While two of these acts involve minor sums of money, all are illegal and all result in personal financial gain for the public official. In terms of our four components we can say that three of the four acts are characterized by the merger of the donor and the public official role. That is, in the cases of DRIVEWAY, TRAVEL, and LAND SALE, the public official is in the position of personally ensuring direct financial gain. Of our ten examples, these are the only three where such a merger is the case, and are the instances of highest consensus. The fourth case perceived as corrupt by over 90 percent is RIGHT WAY. Here the donor is not the public official but an unspecified second party. However, the payoff is very direct and immediate, though in the context of a campaign contribution. We assume that if a campaign contribution were not involved, almost all would have seen the act as corrupt. RIGHT WAY might be compared with another example, OIL, where the payoff from voting a certain way is much more indirect and long-range (i.e., the possible increase in the value of the legislator's stock), and fewer see this type of payoff as corrupt. In sum, from these four examples, we infer that these illegal acts are judged highly corrupt because of the merger of the donor and public official role in three cases, and because of the direct and monetary gain in exchange for a vote in the fourth.

At the other end of the continuum, less than 40 percent of the sample found LAW SCHOOL and the WEAPONS examples corrupt. These acts might be characterized as rather minor forms of influence peddling. In both cases, the payoff is indirect and long-range: good will that perhaps increases the possibility of future campaign support. In both cases the donor is presumably a constituent. The acts themselves obviously are seen by most as routine kinds of favors that public officials try to do for constituents, and thus only a minority are willing to call the acts corrupt.

Our fourfold typology also sheds some light on why there is some ambiguity about these acts. While the payoff is long-range, the donor a constituent, it is implied that doing the favor takes the legislator outside the narrow performance of legislative duty. While the question is unfortunately silent about some aspects of the transaction, it can be presumed that the legislator is calling the law school dean or members of the admissions committee in the one instance, and perhaps putting undue pressure on a bureaucrat in the other. These acts would be considered by some to be overstepping the normal scope of a legislator's activity, but by others as routinely trying to provide a constituent service. Perhaps a clarification of the item would allow a better interpretation.

The acts where there is the least consensus are conflict of interest activities: OIL, DEFENSE STOCK, JUDGE, and AMBASSADOR. In each case, public officials are in the position of furthering personal financial interests while making a decision in their public role. In the case of DEFENSE STOCK, OIL and JUDGE, public officials are in the position of performing a favor (i.e., casting a favorable vote or awarding a contract) for which the payoff is an increase in the value of their own stock. Unlike the DRIVEWAY, TRAVEL or LANDSALE instances, legislators are not the sole donors, although they are in the position of being past donors. Public officials, by their acts, cannot convert favors directly into payoffs. Other events might have an impact on the stock's value, regardless of a public official's favor. And, the payoff is much less direct than a cash grant or immediate service rendered. The government contract, the favorable court ruling, the increased oil depletion allowance may or may not increase stockholders' dividends. On the other hand, the payoff is more tangible than the generalized "good will" engendered by the WEAPONS or LAW SCHOOL acts.

Other factors are also involved in comparing conflict-of-interest acts to the others previously

Table 2. Characteristics of Ten Potentially Corrupt Acts

Act	Public Official	Donor	Favor	Payoff
DRIVEWAY	Political, public role*	Self*	Private*	Large (?)* Specific* Short-range* Noncampaign*
TRAVEL	Political, public role*	Self*	Private*	Large* Specific* Short-range* Noncampaign*
LAND SALE	Political, public role*	Self*	Private*	Large* Specific* Short-range* Noncampaign*
RIGHT WAY	Political, public role*	Unclear ?	Unclear ?	Large* Specific* Short-range* Campaign
JUDGE	Nonpolitical,* public role*	Unclear ? (in part)* Self	Unclear Potential Private* Routine act	Indeterminate size Specific* Long-range Noncampaign
AMBASSADOR	Political, public role*	Constituent	Private* Constituent Routine act	Large* Specific* Short-range* Campaign
DEFENSE STOCK	Political, public role* (?)	Unclear, in part self*	Unclear Potential Private* Routine	Indeterminate size Specific* Long-range Noncampaign*
OIL	Political, public role*	Unclear, in part self*	Unclear Potential Private* Routine	Indeterminate size Specific* Long-range Noncampaign*
WEAPONS CONTRACT	Political, public role*	Unclear, possibly constituent	Unclear, possibly nonroutine*	Small Nonspecific Long-range Noncampaign
LAW SCHOOL	Political, unclear	Unclear, possibly constituent	Unclear, possibly nonroutine*	Small Nonspecific Long-range Noncampaign
CIVIL RIGHTS	Political, public role*	Constituent	Routine Constituent Public	Small Nonspecific Long-range Possibly campaign

*More corrupt.

discussed. The size of the payoff as well as its immediacy influences an official's perception of corruption. We cannot demonstrate that with our data, as all involve $50,000 worth of stock. Beard and Horn (1975), however, offer a relevant example. More Congress members believe it more corrupt for a legislator to cast a vote for benefits to the savings and loan industry when the legislator owns $100,000 worth of stock rather than only $5000 (1975, p. 22). In our own data, several people indicated that if instead of owning $50,000 of stock, the Secretary of Defense were a "major" stockholder, then the taint of corruption might be greater.

Finally, in these four items we can see that the nature of the public office is also relevant. The conflict of interest involving a judge was believed more odious than that involving either a bureaucrat or a member of Congress. A judge, in a nonpolitical role, is held to higher standards than are legislators.

The AMBASSADOR items differ from the OIL, DEFENSE and JUDGE acts. The payoff, a campaign donation, is much more immediate. The donor, however, is providing a campaign contribution rather than funds to be used simply for personal enrichment. To us, the AMBASSADOR and RIGHT WAY cases seem parallel. In one case, the favor is a vote, in another, an appointment. Why the former is seen as more corrupt is undoubtedly because the "spoils system" of appointment has a residue of legitimacy that the practice of "voting for pay" has not.

Our typology of corrupt acts helps explain why these conflict-of-interest behaviors are often ambiguous. As the payoff becomes more and more long-range, and the donor and favors are constituency-oriented, it is easy to argue that an act is not corrupt. Most public officials hope that all acts have some positive payoff for them, principally reelection (Mayhew, 1975), and doing constituency-oriented favors is one way to increase the probability of reelection.[9]

Table 3. Percent Agreeing Act Is Corrupt

	Percent of Respondents Viewing This Act as Corrupt	Percent of Respondents Believing Most Public Officials Would Condemn This Act	Percent of Respondents Believing Most of Public Would Condemn This Act
DRIVEWAY	95.9	92.2	97.5
TRAVEL	95.2	80.4	96.5
LAND SALE	95.1	92.1	97.5
RIGHT WAY	91.9	82.2	94.3
JUDGE	78.8	82.3	91.0
AMBASSADOR	71.1	44.5	81.2
DEFENSE STOCK	58.3	62.1	84.4
OIL	54.9	55.1	81.2
WEAPONS CONTRACT	31.6	20.9	34.4
LAW SCHOOL	23.7	15.5	35.9

Table 4. Correlation Matrix of Perceived Corruption Items

	TRVL	LDSL	RTWY	JUDG	AMB	DFST	OIL	WPNS	LAWS
DRIVEWAY	.19	.28	.28	.06	.12	.13	.02	.03	.09
TRAVEL	–	.29	.35	.07	.15	.08	.10	.10	.10
LAND SALE		–	.33	.16	.15	.16	.06	.08	.10
RIGHT WAY			–	.16	.20	.14	.10	.14	.06
JUDGE				–	.16	.45	.31	.11	.20
AMBASSADOR					–	.19	.23	.15	.22
DEFENSE STOCK						–	.46	.07	.18
OIL							–	.12	.12
WEAPONS CONTRACT								–	.12

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[9] Jack Anderson commented on this point in a recent "expose" of congressional misbehavior: "Favors are part of a politician's stock in trade. He is expected to produce government jobs, public works projects, appointments to military academies and

When the "favor" done for a constituent coincides with personal enrichment, then the charge of conflict of interest is relevant. Clearly our sample is ambivalent and divided about the propriety of these acts.

In sum, the simple rank ordering of our ten examples shows at one end of the continuum a clustering of acts that are clearly illegal or represent a direct financial gain, at the other, acts that are minor influence peddling, and in between a set of acts representing a variety of conflict-of-interest situations. A Guttman scaling procedure revealed that this set of acts was unidimensional. They scaled in the same order as discussed, and produced a coefficient of reproducibility of .95, as shown in Table 4.[10]

Our categorization of the components of a corrupt act aided in pinpointing some of the reasons for differential perceptions of the corruptness of various acts. The nature of the favor, the donor, the public official, and particularly the payoff were all useful in analyzing these perceptions. Acts most perceived as corrupt tended to have different characteristics from those few perceived as corrupt. And, where there was near-unanimity about the corruptness of an act, more components of the act were regarded as corrupt than where there were divided sentiments. Using only these ten acts, it could not be determined which (if any) components, or combinations of components, were crucial in influencing perceptions. More research on that point is necessary. We have only explored reasons why some acts are perceived as more corrupt than others; we have not yet tried to analyze why there are intergroup differences in perception of corruption, particularly differences between elites and the public.[11]

Discussion

Political corruption has not been subjected to the sort of rigorous analysis received by other phenomena in American politics. In large measure, the difficulty of defining "political corruption" accounts for this neglect. The present analysis develops a fourfold classification scheme composed of what we consider the essential ingredients of every act of political corruption. While this scheme is not a "theory," it does offer a conceptual framework helpful in comparing and analyzing potentially corrupt acts. In doing that it allows us to avoid the definitional pitfalls which have stalemated the study of corruption for so long. These problems can be avoided because the components of our conceptual scheme allow one to analyze acts classified as corrupt according to any of the corruption definitions described earlier: the public opinion, public interest, and legalistic definitions.

The scheme, for instance, renders the "public opinion" definition more usable by refining it in such a way as to allow gradients of corruptness in an act and to assist in finding reasons why public officials and elites hold similar or divergent beliefs about a particular corrupt act. This can be accomplished by systematically varying each component of our scheme over a wide variety of acts. The result should enable researchers to pinpoint differences between elites and the mass public, and thus to infer why there are divergent areas—"gray areas of political corruption." Might it be because the two groups view the "payoff" differently: what is seen as small or petty by public officials may be viewed as large and serious to citizens? Or, does the divergence lie in the perception of what is extraordinary in the act providing the favor? Many similar explanations could be posited and tested.

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government contracts can be arranged within the constraints of the law and ethics. But some legislators are not content to wait until election day to bask in the gratitude of the voters."

[10] This correlation matrix of the ten items partially confirms the clustering into three groups though the correlation between the two influence-peddling items is very small.

[11] We have no public opinion data to test hypotheses about elite-mass differences. We do, however, have data on how our respondents think citizens feel about these acts. While the rank ordering or legislators' perceptions of citizens beliefs of corrupt acts is similar to the respondents' belief that most citizens would see every conflict-of-interest act as corrupt (Table 3). The only acts the majority of our respondents believe citizens would not condemn are the WEAPONS and LAW SCHOOL acts. Unfortunately, these data do not test our propositions. They are only suggestive. The respondent's views of citizens reactions to these potentially corrupt acts also form an acceptable Guttman scale, with a CR of .94 and with no items having more than 10 percent error.

One study that surveyed public opinion about corrupt acts was that of John Gardiner (1970), who assessed the attitudes of citizens about political corruption in their community. While he examined tolerance of corruption as it related to several socioeconomic attributes, he did not compare public and elite attitudes. He did find, however, that more educated people were less tolerant of corruption than those with less education.

Moreover, the opportunity now exists to explore possible differences in corruption perceptions based on social class and other subgroups in the population such as race, region, and sex (see Welch and Peters, 1977b). Corruption may also be related to psychological or attitudinal predispositions. For example, "private-" vs. "public-regarding" distinctions sometimes used to explain corrupt behavior can be encompassed within our scheme. For instance, are citizens and elites whose approach to political life is considered "private-regarding" inclined to be less severe in their condemnation of certain political acts than those displaying a "public-regarding" disposition? In this manner, the study of political corruption can be integrated into the broader propositions about the social and attitudinal bases of political behavior.

The scheme can also be related to the definitions of corruption based on "illegal acts" or violations of the "public interest." Are all corrupt acts viewed similarly which are proscribed by law because they deviate from the formal duties of public office for personal gain? If so, the explanation for the common belief may lie in the components of illegal acts such as the payoff, favor, public official or donor. One can view acts considered to be against the public interest or order in a similar way. An act of political corruption may or may not be viewed by all public officials and citizens as violating a responsibility to maintain civic order or as destroying a political system. Given the wide and varying opinions about the "public interest," one would expect they would not. And the four elements, public official, donor, favor, and payoff, allow us to compare and analyze a wide variety of political acts, whether their potential corruptness is based on illegality, violation of the public interest, or condemnation by public opinion. Our approach is also compatible with the "systemic view" of political corruption. If a large part of political corruption in the United States can be attributed to inherent weaknesses in certain offices, situations, or processes, then more honest government will only come about when those weaknesses are identified and eradicated by public officials and concerned citizens. Thus an understanding of individual attitudes is important even when considering the systemic view of corruption.

Inherent weaknesses are difficult to identify. But once exposed they can be analyzed according to our conceptual scheme. Patterns may emerge which would help one predict those offices, institutions or processes susceptible to political corruption. In this manner the sources of political corruption can be systematically identified.[12]

Our approach to defining corruption, then, is offered as a way to integrate research on corruption with the mainstream of research in American politics, a way of integrating theories about corruption to a well-grounded literature in American political beliefs and behavior. This approach also offers a way to study political corruption from the comparative and international perspective. Again the four components are common to every potentially corrupt act. Different hypotheses might be suggested in the cross-national or international study of corruption, but our scheme offers a framework for such a study. For example, a hypothesis might specify that one type of political culture views a certain type of payoff as a routine part of a public official's prerequisites, while another culture views the payoff quite differently. Thus our understanding of differential views toward potentially corrupt acts is enhanced. The citizens of one country may condemn a specific political act as corrupt because it violates the norms of public office holding, while the citizens of another country condone the same act because it does not. The scheme can help us understand why the leaders of a foreign country consider it proper to expect monetary kickbacks from large U.S. corporations in exchange for a government contract, or to offer bribes to key members of the U.S. Congress in exchange for favorable treatment. Moreover, it may well be that stages of economic development, modernization, social infrastructure, and so forth, bear systematic relationship to the components of our conceptual framework. It provides us with another way of demonstrating how political corruption is related to political development.

This approach can also be linked to the more traditional approach of studying corruption only when a significant local or national scandal has occurred. One can monitor public and elite attitudes over time in an attempt to link public attitudes to the situational context.

For too long the systematic study of political corruption has been neglected by serious students of American politics. That this should be so is understandable, considering not only the imposing conceptual problems but also the lack of intellectual respectability about the concept itself. But it has become increasingly evident to citizens, public officials, and scholars alike that "corruption in politics" is just too prevalent a phenomenon not to be subjected to rigorous study. Our article has attempted to demonstrate one approach, but by no means the only approach, to the systematic study of political corruption.

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[12] In our own sample, 35.5 percent believed that there were no specific offices particularly susceptible to corruption; rather, it was a few susceptible individuals (the "rotten apple" view). Of the remaining 64.5 percent, there was little apparent agreement as to which offices were more susceptible to corruption. Most frequently mentioned were offices handling a lot of money, named by 7.3 percent.

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Policy Support within a Target Group: The Case of School Desegregation*

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This study empirically tests three theoretical approaches to explaining specific support for a policy output among members of its target group. The utilitarian *model posits support as a function of objective costs and benefits to the individual stemming directly from the policy. The* attitudinal *model relates specific support to diffuse predispositions rooted in socialization. The* perceptual *model holds that specific support derives from beliefs about the character of the political decision process by which the policy was formulated. Tests of these three approaches are based on survey data on specific support for school district desegregation plans among a large sample of black and white parents of public school children in Florida. In both subsamples, the utilitarian approach explained very little of the variance in support, but the attitudinal and perceptual models were corroborated. Implications of these findings are drawn for desegregation policy making and for public policy theory.*

The implementation of school desegregation policies has sparked bitter and sometimes violent reactions from white parents. Protest marches, boycotts, petitions and in some instances riots and bombings have occurred, and many whites have withdrawn their children from the affected schools. In some districts the white exodus has been large enough to result in resegregation as the public schools became all- or nearly all-black (Beckler, 1971, p. 2; Graglia, 1976, *passim;* Nevin and Bills, 1976, Ch. 2). Nevertheless, behavioral compliance has been the characteristic response of whites to the implementation of desegregation. The overwhelming majority of southern white children now attend desegregated public schools (Farley and Taeuber, 1974; Giles, 1975). Clearly, however, behavioral compliance with a public policy does not necessarily signify subjective support for it. Indeed, surveys and polls report that a large majority of southern whites oppose governmental action to desegregate the schools (Greeley and Sheatsley, 1971; *The Gallup Opinion Index,* 1973; Knapp and Alston, 1972–73).

Given widespread behavioral compliance with a public policy, is subjective nonsupport sufficiently important to deserve analysis by political scientists? Evidence on popular reactions to school desegregation points to an affirmative answer. First, current pressures in Congress to curtail school busing are clearly reactions from those affected by desegregation and who oppose it. Second, Miller and Citrin (1974) suggest that opposition to specific policies may be a source of recent increases in political cynicism among the American public, and report that those opposed to governmental action to desegregate the schools are more often cynics than those accepting action. Third, opposition to desegregation appears to have a "spillover effect" in undermining voter support for increased school funding (Giles, et al., 1976). Finally, parental hostility to desegregation often appears to be transferred to children, leading to disciplinary and educational problems within the schools (St. John, 1975, pp. 64–65 and 89–91). None of this evidence challenges the moral validity of school desegregation in an egalitarian society. However, it reveals that, behavioral compliance notwithstanding, subjective nonsupport may have far-reaching ramifications within the policy process and, therefore, that it merits scholarly attention.

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The purpose of this study is to set forth several possible determinants of subjective support for public policies, and to test them empirically respecting governmental actions to desegregate the schools. In this study, desegregation policy will refer to local school district programs to achieve racially unitary school systems. For, while federal action mandates district officials to carry out desegregation, it is normally the locally designed program that conditions the behavior of the public so as to make desegregation a reality. Second, the target group is here defined as the parents or guardians of children attending public schools in districts that have undergone desegregation. Thus, the dependent variable is subjective support among parents for desegregation programs within the school districts where they reside.

Determinants of Subjective Support

The literature suggests a number of theoretically disparate hypotheses about factors that may affect policy support among members of a target group. In the interests of theoretical clarity, we shall classify them below according to the types of independent variables they employ: (1) objective costs imposed by the policy, (2) long-run attitudes rooted in socialization processes, and (3) short-run perceptions of the political process by which the policy was adopted. While most scholars have restricted themselves to a single approach, all three approaches will be considered here as avenues to explaining parents' support for local desegregation policies.

Policy Costs. Current utility theories of individual decision making portray citizens as rational actors who respond to political stimuli according to their personal costs and benefits; as the ratio of costs to benefits increases, responses become increasingly negative (Downs, 1957, pp. 3–11; Salem and Bowers, 1972; Stover and Brown, 1975). We may infer that as the costs of desegregation policies to parents increases, the support of parents will decline. Utility theories normally assume that people's subjective evaluations of their costs and benefits affect their reactions. However, for the purpose of studying policy impacts, it is more pertinent to understand how support may be affected by variations in the *objective* demands that a policy places upon them. Only in this way can we be sure of distinguishing genuine policy impacts from attitudinal orientations as determinants of support.

School district decision makers across the nation have a common repertoire of three policy elements which they apply to the target group to achieve school desegregation. One is the establishment of black/white attendance ratios for each district school. These ratios may vary somewhat to allow for differences in the percent of blacks in the school-age population by grade levels and the physical capacity of the schools. Second, it is normally necessary to bus many children to overcome residential segregation and to implement racial quotas for individual schools. Third, the spatial dispersion of the schools will require that bused children be transported varying distances from home to school. Therefore, the working hypothesis is that as children experience increasing demands from these desegregation plan elements, their parents' support for the local policy will diminish.

Diffuse Attitudes. An extensive literature on political socialization stresses the importance of diffuse attitudes, often acquired early in life, as determinants of both general and specific support for political objects. (See, for example, Easton, 1965, Chs. 16–20; Almond and Verba, 1963, Chs. 7–9; Dennis, 1973). Three such attitudes will be considered here: racial prejudice, orientations toward the social goals of school integration, and also toward the legitimacy of governmental action to desegregate the schools.

(1) Opposition to school integration among whites is most commonly attributed to racial prejudice (Tumin, et al., 1958; cf. Kelley, 1974). For highly prejudiced whites, desegregation may be a symbolic deprivation even if their children are objectively unaffected by local implementation procedures. Conversely, parents with egalitarian racial attitudes may support the policy even if it drastically alters the conditions of their children's schooling.

(2) A "great debate" continues to be heard concerning the possible effects of desegregation on educational processes (Coleman, 1967; Pettigrew, 1971; St. John, 1975). Prejudice aside, parents' misgivings about the impact of desegregation on school discipline, racial harmony or conflict, and scholastic quality may engender negative reactions to implementation policies. Other parents may be so committed to desegregation as a matter of equal rights and social justice as to submerge any such doubts. In brief, attitudes toward integrating the schools as a social goal may influence parents' willingness to support local policy.

(3) People's general predispositions toward the legitimacy of governmental action in a policy field are said to influence their acceptance of related, specific outputs (Easton, 1965, Ch. 19; Johnson, 1967, Ch. 2). Indeed, Easton suggests (1965, p. 278) that even in the

face of high objective policy costs, support for a policy may be sustained by a strong sense of the moral validity of official action. The hypothesis is that parents' acceptance of local desegregation policies varies directly with their sense of the legitimacy of governmental action to desegregate the schools.

Perceptions of Efficacy and Elite Attitudes. Two major themes recur in the literature concerning the roles of citizens and elites in the policy-making process. One seems rooted in the elitist theory of democracy while the other reflects participatory or populistic theories of citizenship.

Elitist theory has portrayed the mass public as generally ambivalent in its views toward specific policies, except perhaps when the latter directly impinge on personal interests. Hence, the theory stresses the capacity of political leaders to influence mass opinions on policy issues (Dahl, 1961, pp. 164–65 and 264–67; Dye and Zeigler, 1970, pp. 154–61). Some empirical research casts doubt on leaders' ability to shape mass views of specific policies (Lane and Sears, 1965, Ch. 5; Sears and Whitney, 1973). Nevertheless, the assumption of elite influence has pervaded both academic and official publications dealing with school integration (Chesler, et al., 1969; Crain, 1974, p. vi; St. John, 1975, p. 124). In brief, these publications argue that a unified front in favor of integration among local school officials will promote community acquiescence and discourage resistance to desegregation. Therefore, we may hypothesize that parents who perceive their district officials as unified in favor of integration will tend to support local policies, while those who perceive a consensus against integration among local officials will be nonsupportive.

In contrast to the elite influence hypothesis, the populistic view emphasizes the importance that citizens may attach to their active capacity to contribute to the policy-making process. The ability to "have a voice" in the formulation of public policy is said to be valued not only as means of protecting individual interests but also an affirmation of personal autonomy and efficacy (Pateman, 1970, Ch. 2; Thompson, 1970, pp. 13–22). Moreover, empirical evidence from Almond and Verba's five-nation study (1963, pp. 240–53) indicates that the belief that one can participate effectively in the political system heightens satisfaction with its specific performance. In a similar vein, current administrative theory holds that the public's participation in policy formation contributes both to support for the policy and to the achievement of its goals (Rourke, 1976, pp. 147–49). Therefore, we hypothesize that parents' willingness to accept local desegregation policy varies directly with their sense of efficacy in its formulation.

The Data

Data for this study are drawn from a survey of white and black parents of public elementary and secondary school children in seven Florida school districts. Florida has been affected by school desegregation to an extent not found in any other state. Every school district in Florida has experienced desegregation, usually involving extensive busing and mandatory racial balances in all public schools. Moreover, since all school districts in Florida are county-wide, white parents have no escape from desegregation save the private schools. Predictably, political leaders' expressed opposition to desegregation has drawn widespread acclaim throughout Florida. In the 1972 Democratic presidential primary, George Wallace's denunciation of forced integration by federal officials almost certainly helped to gain him a plurality of the vote in every one of the state's 67 counties. In a "straw ballot" in the same election, a majority of voters in every county signified their opposition to busing for desegregation. Also during the 1972 campaign, Richard Nixon advocated "an immediate stop to further new [sic] busing orders by the Federal courts" and "the right of a community to maintain neighborhood schools" (*Congressional Quarterly Almanac*, 1972, p. 51-A). As desegregation took effect in Florida, public protest meetings were held, petitions against desegregation were circulated, sporadic violence broke out and private school enrollments mounted. Nevertheless, the great majority of white parents continued to send their children to public schools. Therefore, Florida presents almost an ideal setting for research on subjective support and non-support for desegregation within a context of maximum application of judicial standards opposed by most whites and their elected leaders.

The 7 study districts were chosen from the 67 in the state to assure variation in urbanization, geographical location, and the percentage of black children in the school-age population. Four of the districts are among the 100 largest in the nation: Dade (6th), Duval (20th), Palm Beach (49th) and Escambia (82nd). Two districts, Lee and Leon, are smaller sized SMSAs, while Manatee is nonmetropolitan. The black population in the districts varies from 19 percent to 33 percent. At the time of the survey, desegregation plans had been implemented in all 7 study districts.

children in these districts was gleaned from official school records, and a total of 2109 white and 1049 black parents were interviewed in the winter and spring of 1973. Thus, all interviewees had had direct experience with desegregation in their school districts. Interview items referred to a single "cue child" in the respondent's family who had been identified from school records and who was referred to by name in the course of the interview. All interviews were conducted in the parent's home by a professional interviewer of the respondent's race.

Respondents' summary opinions of desegregation policy in their school districts were obtained through the question, "In general, how do you feel about the way that school desegregation has been handled around here"? with responses precoded on a five-point scale from "approve strongly" to "disapprove strongly." Responses to this question are presented in Table 1. These percentages cannot be extrapolated to other populations but they reveal sufficient variations in response within both racial groups to permit analysis of some sources of parental support for desegregation policy in their districts.

Table 1. "In General, How Do You Feel About the Way Desegregation Has Been Handled Around Here?"[1]

	White Parents	Black Parents
Approve strongly	2.2%	9.3%
Approve	21.6	46.3
Neither approve nor disapprove	15.8	17.7
Disapprove	29.9	18.4
Disapprove strongly	30.3	8.0
Don't know; not sure	.2	.3
	100.0%	100.0%
Number of respondents	2109	1049
Mean (5-point scale)	2.29	3.31
Standard deviation	1.20	1.17

The percent black enrollment in all public schools in the seven study districts was obtained from official school records and was match-merged with our interview data to permit analysis of parents' responses according to the black/white ratio in the school attended by their child. Interviewees reported whether or not their children were bused to school. Those whose children were bused were also asked to estimate the distance in miles one way. According to policies in all districts, children who are not bused reside within two miles of their public school.

Twelve interview items were designed to measure respondents' racial prejudice, their attitudes toward the social goals of school integration and toward the legitimacy of government action. The prejudice items were omitted in interviews with black respondents. Factor analysis of the attitudinal items produced a three-factor solution for white respondents and two-factor solution for black respondents.[2] The pattern of item correlations with the dimensions was consistent with the hypothesized content of the items. School integration, legitimacy and, for whites, racial prejudice scores were computed for respondents by summing their responses to items loading heavily on the respective factors.

To measure perceptions of local elite consensus, respondents were asked how the "superintendent of schools" and "most of the school board" in their counties felt about school integration. Responses to these questions were precoded (1) "for integration" and (2) "against integration," and were summed.[3] Finally, parents' sense of efficacy respecting local desegregation policy was assessed by the question, "how much influence do you feel people like you have had over school integration in this county?" Responses were precoded on a four-point scale from "a great deal" to "none."

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[1] Immediately following this item, parents were asked the open-ended question, "why do you say that?" Up to three verbatim responses were content analyzed. Approximately two-thirds of each subsample answered in terms of local school district conditions, e.g., busing, attendance zones, the effect of desegregation on education and discipline, and local officials' actions. Less than one-third of all responses alluded to general feelings about school integration. Therefore, the responses in Table 1 are very largely addressed to specific conditions of desegregation rather than casual or diffuse attitudes toward integration.

Also, the responses in Table 1 seem to signify not mere passive acquiescence or disaffection, but rather support or nonsupport in a more positive sense. As responses in Table 1 become increasingly unfavorable, respondents were more likely to report having engaged in protest activities. Also, those at the "strong" ends of the scale were more likely to be "opinion leaders" concerning desegregation.

[2] A report of the factor analysis will be mailed to interested readers, upon request.

[3] There was considerable variation among the seven study districts in the extent to which school officials publicly supported desegregation. The authors compiled a list of 13 supportive activities that officials might undertake–including, for example, publicly stating their intention to implement school desegregation and calling for community support for it. Several expert informants in each district indicated which measures had been carried out locally and which had not. In one district, all 13 (and more) had been undertaken by school officials. In another, only 7 had occurred, while the remaining districts displayed variations between these extremes. Hence, survey respondents had been exposed to a range of differences in leadership activities sufficient to permit meaningful

Data Analysis

Table 2 presents simple (Pearson) correlations between all pairs of variables for the two subsamples, and also reports multiple correlation (R's) for each of the three separate sets of predictor variables. For whites, the three simple correlations between support and desegregation policy elements (percent black enrollment, busing, distance) are the smallest in the table. Moreover, the multiple correlation coefficient for these three variables is extremely low (.176). For blacks, the distance from home to school is the only plan feature having a significant simple correlation with the dependent variable, and the multiple correlation for the three policy applications is approximately the same low figure (.182) as for whites. In both racial groups, desegregation plan elements combine to explain only slightly more than 3 percent of the variance in parents' support.

For white respondents, the highest simple correlations with support are generated by the three attitudinal variables. Support for local outputs increases with their commitment to the goals of school desegregation and belief in the legitimacy of governmental desegregation efforts, and decreases with racial prejudice. Combined, these variables explain almost 30 percent of the variance in support among white parents, far more than is attributable to perceptual variables and plan elements.[4]

In contrast, the overall effect of attitudes on blacks' views appears quite low. The zero-order correlation between legitimacy and support for this subsample is modest but significant (.179). However, the correlation between blacks' scores on the school integration scale and their output support (.084) barely reaches significance at the .05 level, perhaps due in part to the attenuation of blacks' scores on the scale.[5] It follows that erated by these two variables ($R = .443$) is higher than that produced by the other two sets of predictors. The multiple correlations imply that whites' support is less strongly affected by the two perceptual variables than is the case for blacks. Still, they account for about 12 percent of the variance ($R^2 = .116$) in whites' views of the local handling of desegregation. Also, the the total explained variance from the attitudinal variables is low ($R^2 = .034$) for the black subsample.

The sense of efficacy and beliefs about school officials' attitudes toward integration produce the highest simple correlations with policy support among black parents. Also, for blacks the multiple correlation coefficient gen- two perceptual factors produced highly significant multiple correlations (beyond the .001 level) for both subsamples.

In sum, Table 2 suggests that even if it were possible to reduce the objective costs placed on children by the local desegregation policy, little improvement in their parents' approval of the handling of desegregation would follow. The analysis so far implies that policy support is far more contingent on parents' diffuse attitudes and their perceptions of political conditions within their districts. However, Table 2 indicates some substantial intercorrelations between independent variables both among the three theoretical categories and within them. We turn to multiple correlation and regression for all independent variables simultaneously to provide more rigorous tests of the individual hypotheses and to shed further light on the three theoretical approaches.

Main Effects. Table 3 reports the main effects of all independent variables on parents' support in each subsample. We will deal primarily with the standardized regression coefficients (betas) indicating the effect of each independent variable controlling for all the others. For white respondents, Table 3 again attests to the major importance of basic attitudes for policy support. The highest beta coefficients for whites stem from accordance with the goals of integration (.287) and with the right of government to act in the field (.218). Perhaps surprisingly, racial prejudice does not appear to be *directly* related to white parents' policy support once the effects of the other independent variables

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analysis of variations in parents' perceptions of leaders' positions.

[4] Upon excluding the racial prejudice scale, the explained variance for white respondents is reduced to approximately 22 percent. This figure may be more realistically compared to the explained variance for black respondents, since the prejudice scale was not applicable to them.

[5] The school integration scale has limits of 3 and 21. For blacks, that mean score is 19.3 with a standard deviation of 2.8. Thus, the distribution of blacks' scores is somewhat leptokurtic. The mean score for whites is 12.7 with a standard deviation of 5.6.

are controlled. Indirectly, however, prejudice does affect whites' acceptance of local desegregation through its influence on their attitudes toward the social goals of integration and toward the right of government to enter the field. Yet even with the effects of prejudice taken into account, the sense of legitimacy and attitudes toward school integration remain important sources of white parents' acceptance of specific desegregation outputs.[6]

Black parents' predispositions toward the legitimacy of governmental action are substantial contributions to their approval of the local handling of desegregation (beta = .201), but their scores on the school integration scale fail to produce a statistically significant beta coefficient. The latter finding is almost surely due in some measure to the limited variation in black parents' scores of the school integration scale. Because black respondents' commitment to the cause of school integration is so high and so nearly uniform, we cannot safely attribute any of the variance in their policy support to this factor. Therefore, the low multiple correlation in Table 2 between the attitudinal scales and policy support among blacks is probably not a reflection of any inherent weakness in the attitudinal approach per se.

Turning to the perceptual factors, Table 3 indicates that the sense of efficacy in influencing school desegregation decisions contributes significantly to policy support in both subsamples. The statistical findings for both whites and blacks lend credence to the hypothesis inferred from the participatory theory of democra-

Table 2. Simple (Pearson) Correlations for All Pairs of Variables, and Multiple Correlation Coefficients for Theoretical Categories of Variables

	White Respondents									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	R
(1) Local handling of desegregation	—	–.141	–.116	–.121	.184	.308	.503	.463	–.347	
Plan Applications:										
(2) Percent black enrollment		—	.201	.308	.125	–.094	–.176	ns	.179	.176***
(3) Busing			—	.363	ns	–.093	ns	ns	.096	
(4) Distance				—	ns	–.096	ns	–.095	ns	
Perceptions:										
(5) Elite consensus					—	.131	.199	.200	–.178	.341***
(6) Efficacy						—	.298	.302	–.190	
Attitudes:										
(7) School integration							—	.583	–.570	.547***
(8) Legitimacy								—	–.141	
(9) Race prejudice									—	

	Black Respondents								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	R
(1) Local handling of desegregation	—	ns	ns	–.170	.398	.250	.084	.179	
Plan Applications:									
(2) Percent black enrollment		—	ns	ns	ns	ns	ns	ns	.182*
(3) Busing			—	.358	ns	ns	ns	ns	
(4) Distance				—	–.085	ns	ns	.090	
Perceptions:									
(5) Elite consensus					—	.143	ns	ns	.443***
(6) Efficacy							ns	ns	
Attitudes:									
(7) School integration							—	.214	.184**
(8) Legitimacy								—	

Significance (F-test): * = .05
** = .01
*** = .001

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[6] A further regression, not reported here, deleted the prejudice scale but retained all other independent variables. The result was to decrease the beta coefficient for whites on the school integration scale fo .313, while the betas for all the remaining variables, as well as the total explained variance, remained virtually identical to those reported in Table 3.

cy—that the sense of having a voice in the making of a public policy is a strong incentive to support for it.

However, there is little evidence in our data of the "follower mentality" predicated by elitist theory. Most of our respondents in 1973 had ignored, forgotten, or misconstrued Richard Nixon's anti-integrationist stance in 1972. For 70 percent of the whites and 31 percent of the blacks perceived "the president" as *favoring* school integration, 14 percent of the whites and 30 percent of the blacks saw him as against integration and the remainder reported not knowing his position. Further, past research (Lane and Whitney, 1973, pp. 8–10) indicates that people often project their own views upon political leaders. To the extent that projection occurs, leaders cannot be said to exert genuine influence on mass opinions. Many whites in our sample appear to have projected their attitudes toward school integration on the president, for the simple correlation between the two variables was modest but significant ($r = .113$). Further, the beta coefficient for perceptions of the president was nonsignificant for whites. For blacks, the corresponding beta was significant. However, blacks' attenuation on the school integration scale precluded a test of the projection hypothesis. The safest assumption is that basic psychological processes operate similarly in both groups. It seems unlikely, then, that blacks were any more open to influence from the president than were whites.

The evidence is not much more impressive respecting local school officials' influence on parents' support. As was the case regarding the president, whites frequently appear to project their predispositions upon local school officials, for Table 2 reports significant simple correlations between whites' scores on the school integration, legitimacy, and race prejudice scales and their perceptions of district officials' positions. Still, controlling for whites' attitudes, their perceptions of local officials make a significant yet extremely slight contribution (.061) to the total variance in their support.[7]

For blacks, perceptions of school officials' views toward school integration produce the strongest beta coefficient in the equation (.377). Nevertheless, it is hard to accept the

Table 3. Multiple Correlation and Regression Analysis of Support for Desegregation Policy

	White Respondents				Black Respondents			
	(b)	(beta)	(s)	(F)	(b)	(beta)	(s)	(F)
Independent Variables:								
Percent black enrollment	–.106	–.040	.068	2.416	–.008	–.059	.004	2.720
Busing	–.123	–.051	.063	3.845*	.018	.007	.091	.041
Distance bused	–.094	–.032	.078	1.477	–.510	–.153	.129	15.686***
School integration scale	.049	.287	.006	73.728***	.210	.056	.014	2.315
Legitimacy scale	.041	.218	.006	51.974***	.040	.201	.007	29.591***
Race prejudice scale	.009	.044	.006	2.201	––	––	––	–––
Perceptions of local elites	.098	.061	.040	5.945*	.492	.377	.048	107.404***
Perceptions of efficacy	.108	.128	.036	24.984***	.224	.185	.044	25.820***
Dependent Variable								
Mean (5-point scale)	2.290				3.315			
Standard deviation	1.218				1.172			
F-ratio	71.393				29.639			
Standard error of estimate	.990				1.013			
Number of respondents	1184				581			
R	.572				.516			
R^2	.327				.266			

F-test:
***$p < .001$
**$p < .01$
*$p < .05$

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[7] For whites, the projection hypothesis was tested more rigorously by examining the difference in explained variance between a restricted regression model, including all independent variables except perceptions of officials' positions, with a full model incorporating the perceptual factor and all other independent variables. Since the restricted model permits attitudinal forces to make their fullest contributions to the explained variance, any additional variance explained by the full model can be attributed to the unique influence of perceptions of officials. This test revealed that for whites, the full model produced an explained variance of only .037 in excess of that from the restricted model, although this difference was unlikely to be due to chance. For a fuller explication of this procedure, see Wright (1976).

notion that, after generations of discrimination, blacks have become susceptible to influence from the predominantly white school officials in our study districts. The strong beta may have other sources.[8] It might be supposed that blacks whose children bear a heavy objective burden from the desegregation plan feel that local decision makers were hostile to racial integration. This assumption would be consistent with the significant negative correlation for blacks (Table 2) between busing distance and perceptions of district officials. And, as noted previously, increasing distances tend to depress blacks' approval of the handling of desegregation in their districts. Hence, notwithstanding the statistical evidence in Table 3, it remains questionable whether black parents' support for local policies was molded by their school district officials.

White and black parents appear to react somewhat differently to the three objective policy elements. For neither subsample does the black/white attendance ratio in the child's school produce a significant beta coefficient. Busing per se has a slightly negative effect on whites' but not on blacks' approval of local outputs. Among blacks whose children are bused, however, an increasing distance appears to depress policy support. We turn to a closer examination of these policy impacts below.

Interaction Effects. The impacts of objective policy elements are not fully tested by the regression model used so far, for it estimates only the individual additive effects of variables. However, the plan elements are applied to many children in combinations. Thus, while a high percent black enrollment seems to have little influence on support, it may become more important when it occurs together with busing and a long distance from home to school. Therefore, a series of variables was constructed to specify all second- and third-order interactions among the three plan elements, and were added to the main effects regression model presented in Table 3.[9]

For whites, none of the interaction terms produced a statistically significant beta coefficient. That is, over and above busing per se, a longer bus ride or an assignment to a heavily black school, *or both*, have no further depressing effects on whites' support. Hence, the conclusion from Table 3 is reconfirmed—that busing as such is the dominant policy application influencing white parents' policy support.

For blacks, however, clear and strong evidence of policy influence on support emerges from the inclusion of the interaction terms. Preliminary analysis (not reported) revealed that one such term, representing the simultaneous application of all three plan elements, was statistically significant for blacks. Table 4 presents the final regression solution for these parents including the significant interaction. The third-order interaction term is presented conditionally with the effects of the percent black enrollment examined within categories of busing and distance.[10] Table 4 shows that among blacks whose children are bused a long distance, an increase in the percent black enrollment strongly diminishes support (beta = −.335). However, increases in the percent black have no effect among those bused a short distance.[11] Moreover, with the combined effect of busing and percent black specified, the main effect of the distance bused is reduced to statistical insignificance. Apparently, then, the linkage between distance and policy support in Table 3 stemmed from low support among black parents whose children were bused a long distance to schools with higher percentages of black pupils.

Why does the support of blacks decline so precipitously when their children are bused a long way to heavily *black* schools but not to heavily white schools, and why is their support unaffected by a shorter bus ride to a school with many black children? Upon reflection, these findings are not surprising. For the black children in question are not only bused, but also they must travel a long distance, only to disembark at schools very similar in racial composition to those in the neighborhoods where they boarded the bus. To school district planners, this combination of policy elements may seem warranted as part of a rational strategy for achieving a federally acceptable distribution of black and white children among local schools. To black parents, however, this pattern may well appear both burdensome and pointless.

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[8] Perhaps the very fact that desegregation had taken place in our study districts made some black parents presume that local officials favored school integration. The hypothesis could not be tested since our sample does not include blacks in districts that have not been desegregated. However, even if the hypothesis were validated, it would not necessarily mean that blacks' perceptions of officials determined their support for district outputs.

[9] Interaction terms were obtained by multiplying the appropriate policy variables.

[10] That is, the percent black enrollment is allowed to vary within the fixed categories of distance among bused children.

[11] Similarly, the lack of significant interactions indicates that all other combinations of policy impacts are also nonsignificant.

Altogether, the regression analyses have accounted for about one-third of the variance of whites' support and approximately one-fourth of the variance in blacks' support. These figures may seem respectably high given that the analysis has been restricted primarily to hypotheses drawn from empirical political theory. Perhaps the addition of some sociological or demographic variables might inflate the multiple correlation coefficients. In any case, the research goal has not been to maximize overall predictive power but rather to assess the relative utility of several political science approaches to explaining policy support in a target group.

Discussion

We began by setting forth three theoretical approaches to explaining specific support for a governmental policy among its target group members. The first suggests that support will vary with the objective costs and benefits of the policy. The other two approaches focus upon subjective factors—diffuse attitudes and perceptions of the policy-making process—as determinants of support. Our analyses leave a deep impression of the weakness of the cost-benefit approach and the strength of subjective factors in explaining both black and white parents' support for school desegregation outputs in their communities.

The cost-benefit assumption is firmly grounded in utility theories of individual behavior current in political science and economics. Furthermore, policy makers themselves appear to believe that the objective demands of policies will affect a target group's reactions and try to anticipate "what the market will bear" in formulating outputs. In this light, our most intriguing finding is a negative one—that the objective demands placed upon school children by desegregation have, at most, only very slight impact on parental support for local policies. Busing is the only policy element that depresses whites' support, while only the most extreme combination of all three elements diminishes blacks' support. Nevertheless, these policy demands pale in causal importance when compared to the effects of subjective attitudes and perceptions of parents' acceptance of the handling of desegregation in their school districts. Our findings underscore Edelman's contention (1960, passim) that the costs and benefits of a policy may be more symbolic than tangible.

It is hardly surprising to find that increasingly favorable attitudes toward the goals of school integration generate increasing approval of district desegregation policies. However, target group support is not simply a function of agreement with the general purposes of the policy. It is also rooted in parents' appraisal of two political objects. First, our analyses corroborate recent theory and research indicating that a diffuse sense of the legitimacy of governmental action in a policy field stimulates specific support for related outputs. Second,

Table 4. Black Respondents: Interaction Effects of Desegregation Plan Applications

	(b)	(beta)	(s)	(F)
Independent variables:				
Perceptions of local officials	.497	.381	.047	110.85***
Legitimacy scale	.040	.204	.007	30.93***
Perceptions of efficacy	.225	.185	.044	26.30***
Bused, long distance, and percent black enrollment	–.688	–.335	.221	9.73**
Bused, short distance, and percent black enrollment	–.007	–.057	.005	2.51
School integration scale	.021	.056	.014	2.33
Distance bused	.528	.158	.360	2.15
Busing	–.033	–.014	.090	.13
Multiple correlation:				
F-ratio	27.509			
Standard error of estimate	1.001			
Number of respondents	580			
R	.527			
R^2	.278			

F-test:
***$p < .001$
**$p < .01$
*$p < .05$

participatory theories of democracy stress the value that citizens may attach to their ability to make effective contributions to the making of policies directly affecting them, and our data bear out this assumption. Parents' beliefs about their efficacy in influencing desegregation policy making in their districts were a strong contributor to their support. Diffuse predispositions aside, specific support is unlikely to be maximized when the decision-making process is widely viewed as insulated from citizen influence.

However, by the most generous interpretation, our analyses lend only tenuous confirmation to the elitist assumption that public officials shape the target group's reactions to desegregation policies. There was no convincing relationship between parents' perceptions of the president's position on the integration issue and their acceptance of local policies. Moreover, contrary to much of the topical literature, local school officials seem generally unable to induce a recalcitrant white public to accept desegregation. Whites' perceptions of district officials' attitudes toward school integration often seemed to be simply projections of their own views. Once these projection effects were taken into account, the perceived position of local school officials had only trace effects on whites' support.

At first glance, blacks' perceptions of district officials' positions on the integration issue appeared to be a strong statistical predictor of their policy support, and there was no evidence that blacks projected their general enthusiasm for school integration upon local officials. Nevertheless, some of the data implied that blacks' satisfaction with district policies may be a determinant of their perceptions of officials, rather than vice versa as the elitist hypothesis holds. Therefore, it remains doubtful that blacks are any more open to elite influence than are whites.

These conclusions are parallel to Hamilton's finding (1970) that political and social elites were unable to arouse mass support for open housing measures, and at odds with Mueller's finding (1973, pp. 69–73, 123) that the mass public normally has played a follower role vis-à-vis the president on foreign policy issues. Thus, elite influence may vary according to the issue. School desegregation, like open housing, directly affects the everyday lives of target group members and engages long-standing attitudinal commitments concerning race relations. Given these conditions, the individual probably has a sufficient personal framework for assessing policies without resorting to cues from elites. On the other hand, when issue objects are more remote (e.g., foreign policy) or when fundamental values are not threatened, the assumption of elite influence may yet hold true.

Black and white respondents have been analyzed separately because it was initially supposed that the sources of support might be different for the two groups. However, this does not appear to be so. Some seeming differences between blacks and whites were almost surely due to technical difficulties in measuring black parents' attitudes (i.e., the lack of a valid scale of blacks' stereotypes of whites and the attenuation of blacks' scores on the school integration scale). Hence, policy makers probably face much the same kinds of problems in stimulating support for desegregation in both racial groups.

It is hard to be optimistic about the prospects for improving whites' views of desegregation policies, at least in the short run. For our analyses portray a target group whose policy support is largely rooted in fundamental attitudes which probably change slowly. There is virtually no evidence in these data that district officials can increase support *either* by minimizing the objective costs of desegregation to parents *or* by presenting a united front in favor of school integration. Only an "interactive" leadership strategy appears to hold much promise—i.e., one that facilitates citizen contributions to district policy making, that accommodates citizens' wants to realistic policy alternatives, and that reduces the social distance between target group members and policy makers. The data imply that strategies of this nature might generate a somewhat broader acceptance of local desegregation outputs. Their costs may be far less than the costs of continued public nonsupport, the ramifications of which were suggested in the beginning of this study.

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Analyzing Diffusion and Contagion Effects: The Urban Disorders of the 1960s*

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This study concerns the analysis of diffusion and contagion processes using a lognormal model of overdispersion phenomena. The urban disorders of the past decade are examined and two processes are found to exist in the 1966–67 period. One is a classic diffusion effect in which disorders are precipitated by events which are independent of each other, but lead to outcomes such as numbers of arrests which are proportional to previous disorders. The second process is a contagious one in which disturbances occur as a consequence of smaller cities imitating the behavior of large ones experiencing a disorder. It was found that the explanatory power of the interaction effect between police and black city residents tended to increase as city size increased. Concomitantly, the effects of environmental variables tended to decrease in explanatory power as city size decreased.

Recently, there has been a renewed interest in the diffusion of social phenomena. Policy innovations in the areas of health, professional licensing, conservation of energy, and civil rights all have received extended treatments in the context of the American states, while additional processes such as social security adoptions and political instability have been examined cross-nationally. In these studies, the diffusion process typically has been analyzed by means of a hierarchical framework. It was found, for example, that larger and wealthier states tended to adopt innovative social programs more quickly than smaller states (Walker, 1969). Cross-nationally, the adoption of social security programs was found to depend on a hierarchy of modernization (Collier and Messick, 1975), as the diffusion of instability in Latin America depended on the hierarchical diplomatic status of a country (Midlarsky, 1970; Li and Thompson, 1975). Galton's problem as a related issue concerning the statistical independence of units for inferential purposes, also has received recent attention (Gillespie, 1970; Naroll, 1973; Ross and Homer, 1976).[1]

A major exception to this pattern of findings has been the statistical analysis by sociologists of the urban disorders of the 1960s.[2] Several analyses of these disorders (Spilerman, 1970, 1971, 1972) found no appreciable diffusion or contagion effect, and found the only variable that was significantly related to the occurrence of the disorders to be the size of the urban black population.[3] Although the size of the police force and total urban population also were found to be related to the frequency of the disturbances (Morgan and Clark, 1973), more recently, the size of the black population once again was stated to be the primary substantive variable of importance (Danzger, 1975). Further, none of these studies employed a theory of diffusion or even a simple model of the diffusion process.

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[1] Much of the literature on diffusion, excluding political science, is referenced in Rogers and Shoemaker (1971). Many of these works refer to hierarchical diffusion processes in fields as diverse as fluoridation and electronic technology.

[2] Political science studies which have focused on urban unrest include those by Lipsky (1968), Eisinger (1973), and Lupsha (1969), largely from the perspective of collective protest, or theories of collective violence. Sears and McConahay (1973) are more directly concerned with the violence itself, especially that associated with the Watts riot of 1965.

[3] Although Spilerman (1970) found some modest evidence for geographical contagion, he largely rejected that possibility, and subsequently (Spilerman, 1972) reinforced that rejection rather strongly. Mazur (1972) in a comment on Spilerman's work essentially disputed the substance of Spilerman's findings on contagion.

This conclusion that a diffusion effect was absent is unsatisfactory (1) because of the lack of the use of a testable theoretical framework for the explanation of a diffusion process, and (2) because the absence of a diffusion effect in no way explains the steady increase in the number of disorders between 1965 and 1968. Another problem associated with previous analyses is the lack of any indication of change in the frequency of the disorders. The absence of longitudinal analyses in statistical studies of disorder frequency is a serious shortcoming.

Given this state of affairs, it seems advisable to approach the analysis using a different analytical perspective and data base. Spilerman initially analyzed the disorders of this period by means of the negative binomial distribution primarily because of its empirical use as a descriptor of over-dispersion phenomena.[4] In this discussion, we will use a model of proportionate effect and later a hierarchical status effect to analyze the disorder data. Rather than cover the entire period, 1961–68, as did Spilerman, we will analyze a more restricted 1966–67 period, using a data base which is not subject to the criticism of the over-reporting of conflict events.[5]

A major purpose of this study, however, is not simply to reanalyze the disorder data of the 1960s. It is also to introduce a method of analyzing diffusion phenomena that has been relatively common in the biological sciences and in economics. In particular, we will use the lognormal distribution as a means of analyzing overdispersion phenomena that have resulted from diffusion processes. As will be seen, a major differentiation will emerge between what may be reasonably called a diffusion effect and a more intense variant of the same type, contagious processes. Further, we will develop a hierarchical model in which the diffusion of behaviors of a certain type occurs in large prototype cities, but not in smaller ones which are subject to contagious phenomena.

The Lognormal Distribution

The lognormal distribution generally has been used as a distribution model of growth and comminution processes. Both the growth of personal income or of gross national product have been described by the lognormal distribution, and the rendering of particles into ever smaller particle sizes also has been described by this distribution (Aitchison and Brown, 1957; Cramér, 1946). More directly, the lognormal distribution is the solution to a particular variant of the basic Kolmogorov diffusion equation that has been applied to the stochastic growth of national economies (Tintner and Sengupta, 1972; Bharucha-Reid, 1960). It is also the distribution with the highest degree of skewness and leptokursis of the frequency distributions normally considered and therefore is most capable of modeling extreme over-

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Note that the preceding analyses by Spilerman concern the frequency of disorders, which, of course, is the subject of the present treatment. Later, in a study of disorder severity, Spilerman (1976) did imply the possibility of a contagion effect but did not systematically develop this idea. As in his earlier studies of disorder frequency, he finds the size of the black population once again to be the only structural characteristic of cities to be significantly related to an important property of the disorders.

[4] Spilerman (1970) confirmed the nonapplicability of the Poisson for the period 1961–1968, as did Lieberson and Silverman (1965) for the 1913–1963 period, and at the same time he found a good fit between the negative bionomial distribution as a measure of overdispersion and the disorder data. Either diffusion processes (including reinforcement) or heterogeneity of the cases studied can lead to the applicability of this distribution (Feller, 1943). He then rejected diffusion or geographic contagion as an explanation for this finding, and opted for heterogeneity of urban black population size as the preferred explanation for the applicability of the negative binomial.

Other works which have employed related distributions such as the Poisson, include McGowan and Rood (1975), Job (1976) and Siverson and Duncan (1976). Among the earliest treatments of contagion are those by Feller (1943) and Newbold (1927), while S-shaped treatments of diffusion phenomena are found in, among others, Kuznets (1930), Coleman et al. (1957) and Gary (1973).

[5] The source is *Riots, Civil and Criminal Disorders* (1967) and includes only cities which experienced one or more disorders. This is precisely the condition under which Danzger (1975) found a dramatic reduction in the effect of over-reporting by the wire services (AP/UPI) on the data (from 28 percent to 7 percent of the controlled variance). Thus the data source automatically controls for the single largest contaminant of the disorder data found thus far. It should be noted that multiple disorder cities may behave differently from those experiencing single disorders because the processes of reinforcement would be applicable here. However, the relatively few multiple disorder cities are large ones (e.g., New York, Chicago) and as will be seen, the major contagious processes take place between large and small cities, thus obviating the reinforcement process. This source has exhibited some considerable systematic variation of the data in prior studies of the disorders. See, for example, Wanderer (1969).

dispersion phenomena (Anscombe, 1950; Cassie, 1962).[6]

An underlying basis of the applicability of the lognormal distribution is a proportionate effect diffusion process. A particular phenomenon such as a series of disorders expands over time via a mechanism in which each additional disorder increment is proportional to the existing size of the process. Precipitating events occur at random, independently of each other in separate cities, but the response in each case (a disorder) is some random proportion of the set of preceding responses (prior disorders). Examples of such mutually independent precipitating factors occurring in separate cities, given in fact in the data source, are the police shooting of a black male fleeing after an arrest, or a fight between black and white youths, which led to police involvement.[7]

We can let $\{\mu_i\}$ be the set of mutually independent random precipitating factors, and $\{x_i\}$ be the set of disorder responses in the form of number of arrests. All of the disorders are characterized by some nonzero value of arrests, while many of the disorders incurred zero or at least very small values of the number of killed or injured as another possible descriptor of the disturbances. Thus, the variation among the disorders is maximized by the choice of the variable, number of arrests. By the reasoning concerning the proportionate effect response, the incremental change $x_i - x_{i-1}$ should be some random proportion of the existing value, x_{i-1}, or

$$x_i - x_{i-1} = \mu_i x_{i-1}, i = 1,2, \ldots, n. \qquad (1)$$

Equation (1) states that the change in the number of arrests as the result of an additional disorder is proportional to the size of this variable at the time the disorder occurred. It can be shown[8] that equation (1) leads directly to the fundamental equation underlying the lognormal distribution,

$$\log x_n = \log x_o + \mu_1 + \mu_2 + \ldots + \mu_n. \qquad (2)$$

By the additive form of the central limit theorem, log x_n is normally distributed in the limit and, as a result, x_n is lognormally distributed according to the equation

$$f(x) = \frac{1}{x\sigma\sqrt{2\pi}} \exp\left[-\tfrac{1}{2}\left(\frac{\log x - m}{\sigma}\right)^2\right]$$

$$x > 0 \qquad (3)$$

where m is the mean of the logarithm of x and σ is the standard deviation of that variable.

A test of the applicability of the lognormal distribution is found in Table 1 for 1966–67. We use a table to illustrate the actual process of applying this distribution, but figures will be employed subsequently for ease of presentation. The variable x_i is the number of arrests in each of the disorders, and we choose the logarithm to the base 2 for convenience to allow for the counting of the number of disorders in each of the arrest categories 2^2 (4–7), 2^3(8–15), . . . , there being no disturbances in the data source with fewer than four arrests.[9]

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[6] Anscombe (1950) has ranked a series of overdispersion models based on increasing skewness and tail length, and the lognormal had the greatest degree of skewness and leptokursis of the frequency distributions normally considered. Some distributions which are more highly skewed and leptokurtic than the lognormal are, however, highly intractable in their application. Certain distributions such as the Pareto Type II (or Pearson Type XI) are J-shaped and some do not even have a mean. The lognormal distribution, of course, has all moments existing, but the distribution with density

$$f(x) = \frac{q}{c}\,\frac{1}{(1+\frac{x}{c})^{q+1}}\ (x \geqslant 0)$$

has varying numbers of moments depending on the value of q. I am grateful to Professor David Bartholomew of the London School of Economics for suggesting this particular distribution form in a personal communication. For a treatment of relatively uncommon distributions such as the Pearson Types VIII to XII see Elderton and Johnson (1969, pp. 45, 85–97). The relationship between the Pareto Type II and the more general Pearson Type VI is found in Johnson and Kotz (1970, p. 234).

[7] See *Riots, Civil and Criminal Disorders* (1967).

[8] Assuming approximately that the distances $x_i - x_{i-1}$ are very small in equation (1), dividing by x_{i-1}, summing and then integrating yields equation (2), as in Aitchison and Brown (1957, pp. 22–23). It should be noted here that the μ_i are independent random variables (unitless as can be seen in equation [1]), and so would assume different values in different settings depending on the circumstances at hand.

[9] For convenience and clarity of presentation, the beginning point of each range of arrests is chosen as the basis for representing the logarithmic value (e.g., for the 8–15 category, $\log_2(2)^3 = 3$), rather than the midpoint. This, of course, does not change any of the conclusions, since all of the categories are similarly affected.

The parameters m and σ^2 were estimated by the equations

$$m = \frac{\sum_{i=1}^{n} \log_2 x_i}{n} \tag{4}$$

and

$$\sigma^2 = \frac{\sum_{i=1}^{n} (\log_2 x_i - m)^2}{n-1} \tag{5}$$

where the variable $\log_2 x_i$ is now assumed to be normally distributed, from equations (2) and (3), and the usual table of areas under the normal curve was used to calculate the theoretical values shown in Table 1. A reading of the theoretical values suggests that the fit is not particularly good, with the observed values demonstrating a considerable degree of positive skewness. Observed values in the lower portion of the disorder data are underrepresented theoretically, while those in the upper portion are overrepresented. It is not likely that the χ^2 goodness of fit statistic occurred by chance, given its low probability value, which also suggests the lack of direct applicability of this distribution.[10]

It is now evident that the distribution which represents the greatest degree of overdispersion still is not capable of representing directly the disorder properties of this period. The degree of overdispersion is greater than the lognormal distribution can represent and, as noted earlier, this is the distribution with the greatest degree of skewness and leptokursis of any of the frequency distributions normally considered. Thus, it is uniquely suited to provide a mathematical representation of extreme overdispersion, and yet cannot do so.

There are at least two possible reasons for this outcome; either (1) the lognormal is simply the wrong model for the representation of the disorders and they are so hopelessly overdispersed that there is no possibility of mathematically describing them with any degree of parsimony and simple structure, or (2) there are at least two processes at work here such that their combination renders any single description ineffective. Note that the fit of the lognormal distribution in Table 1 is not entirely askew and this suggests further exploration of the second possibility, namely the two-process hypothesis. In order to continue this line of inquiry, I will now turn to a longitudinal analysis of the disorders in the years 1966–67.

A Longitudinal Analysis

In order to assess the patterns of the disorders over time, we require some method of presenting them in a longitudinal analysis. If the disorders occur in a random sequence, then the sum of characteristics of the disorders, such as arrests, should approximate a straight line. Using either a logarithmic or a linear presentation, this can be seen in the following manner,

Table 1. Observed and Lognormally Predicted Distributions of the Number of Arrests, 1966–67

Range of Arrests x		$\log_2 x$	Observed Frequency of Disorders	Expected Frequency of Disorders ($m = 5.3646$, $\sigma = 1.8360$)
2^2	(4–7)	2	4	3.19
2^3	(8–15)	3	9	6.31
2^4	(16–31)	4	19	12.46
2^5	(32–63)	5	21	18.49
2^6	(64–127)	6	23	20.55
2^7	(128–255)	7	8	17.10
2^8	(256–511)	8	8	10.65
2^9	(512–1023)	9	1	4.97
2^{10}	(1024–2047)	10	2	1.73[a]
2^{11}	(2048–4095)	11	0	.45
2^{12}	(4096–8191)	12	1	.10
			96	96.00

$\chi^2 = 14.319$, $df = 6$, $.02 < p < .05$

[a] The last three categories are combined for the chi-square test.

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[10] Adjacent categories are combined under conditions where one category has an expected value less than 1.5. This value is suggested in Gibbons (1971, p. 72). The number of degrees of freedom is equal to $k-3$ where k is the number of categories and one degree of freedom is subtracted for each of the three restrictions on the theoretical distribution, namely, that it have the same mean, variance, and total number of cases as the observed distribution.

the logarithmic mode being used here for convenience in illustrating the process. A linear representation would lack the smoothing effect of the logarithmic values. As before, let the x_i be the number of arrests and the μ_i be the random precipitating incidents. Now let

$$x_1 = k_1 e^{\mu_1}, x_2 = k_2 e^{\mu_2}, \ldots, x_n = = k_n e^{\mu_n}, \tag{6}$$

where k_i is a constant of proportionality between arrests and the exponential function. In this fashion, each of the equations expresses a logarithmic relationship between the variant and precipitating agent, i.e.,

$$\log x_1 = k_1' + \mu_1, \log x_2 = k_2' + \mu_2, \ldots, \log x_n = k_n' + \mu_n.$$

Multiplying each of the equations (6) by the other yields

$$x_1 x_2 \ldots x_n = k_1 k_2 \ldots k_n e^{\mu_1 + \mu_2 + \ldots + \mu_n}. \tag{7}$$

Taking logarithms of both sides of equation (7) gives

$$\log (x_1 x_2 \ldots x_n) = k'(\mu_1 + \mu_2 + \ldots + \mu_n),$$

or,

$$\sum_{i=1}^{n} \log x_i = k' (\mu_1 + \mu_2 + \ldots + \mu_n). \tag{8}$$

If the precipitating incidents occur in time sequence such that μ_n occurs later than μ_{n-1}, ..., then the μ_i are time-ordered, and it follows directly from equation (8) that

$$\sum_{i=1}^{n} \log x_i = k''t, \tag{9}$$

or the summation of the logarithms of the arrests is proportional to time. The cumulative logarithms of the number of arrests for 1966 are plotted versus time in Figure 1 (dotted line portion).[11] It approximates a straight line, as suggested by equation (9).

Now, this process can be examined for the year 1967. If the same process applied in 1967 as it did in 1966, then the same straight line should appear. Figure 2 plots the cumulative logarithms of the number of arrests for 1967; the pattern overall is markedly different. The first portion of the curve between April 1 and July 12, and the latter between August 8 and September 7, are approximately the same in form and, in fact, are quite similar to the curve for 1966 in Figure 1. As the result of differences in scale, this similarity cannot be seen directly from Figure 2, but an expanded version from June 3 to July 9 appears in the upper left-hand portion of Figure 1 for purposes of comparison. The average number of incidents for this 1967 period (.25 disorders per day) is virtually identical to that obtained for 1966 (.21 disorders per day). However, the mid-portion of the 1967 plot, roughly from July 14 to August 3, shows a very rapid increase, with an average equal to 2.75 disorders per day. This is a substantial departure from the previous pattern.

The similarity between the first and the last portions of Figure 2 and the pattern shown for 1966 in Figure 1 suggests that these are one and

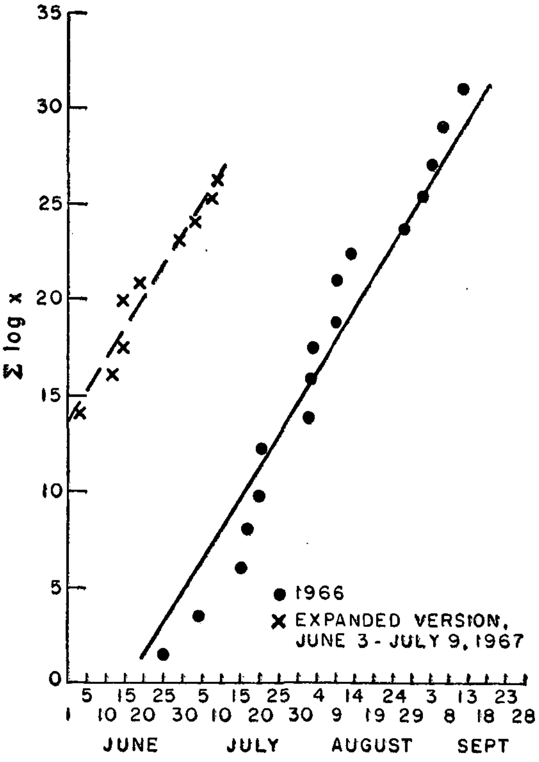

Figure 1

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[11] The logarithm to the base 10 is used here for convenience, given the ready availability of such values carried out to large decimal places, although of course any logarithmic base ($b > 1$) would give the same plot of relative time spacing of the disorders. In addition, in order to allow for the possibility of diffusion effects, a disorder had to occur within a two-week period of other disorders in order to be included in the plot. As a result, a total of four disorders, two at the beginning of the disorder period and two at the end, were excluded from the 1966 plot. All of the 1967 disorders recorded in the source met this criterion.

the same process. On the other hand, the time period in the center portion of Figure 2, and which shows the rapidly accelerating increase, appears to be a different process. These may, in fact, be the two separate processes which contributed to the overall nonapplicability of the lognormal distribution in Table 1.

Treating now the similar processes of 1966 and the first and last portions (parts A and D) of 1967 together, Figure 3 shows the frequency distribution of the arrests corresponding to these disorders. The fit is a good one visually, and the χ^2 goodness of fit value now has a much higher probability of occurring by chance. There is still some overrepresentation in the observed data for the smaller categories, but not nearly as severe as in Table 1 which represents the entire period. Figure 4 shows the observed and lognormally predicted values for the arrests in the period July 14 through August 3 (sections B and C) of Figure 2. The fit is much worse, as would be expected given that this period was absent from the analysis leading to Figure 3, and a better fit of the lognormal distribution was obtained in that instance. Here, the overrepresentation is apparent in the lower values, with the higher values of arrest categories having a very low representation in the disorder data. There is a clustering of arrests in the categories 4–6 in Figure 4, a factor which is largely absent from Figure 3 in which the theoretical contours of the lognormal distribution generally follow the observed data. The period demarcated by section C alone, also deviates from lognormality to

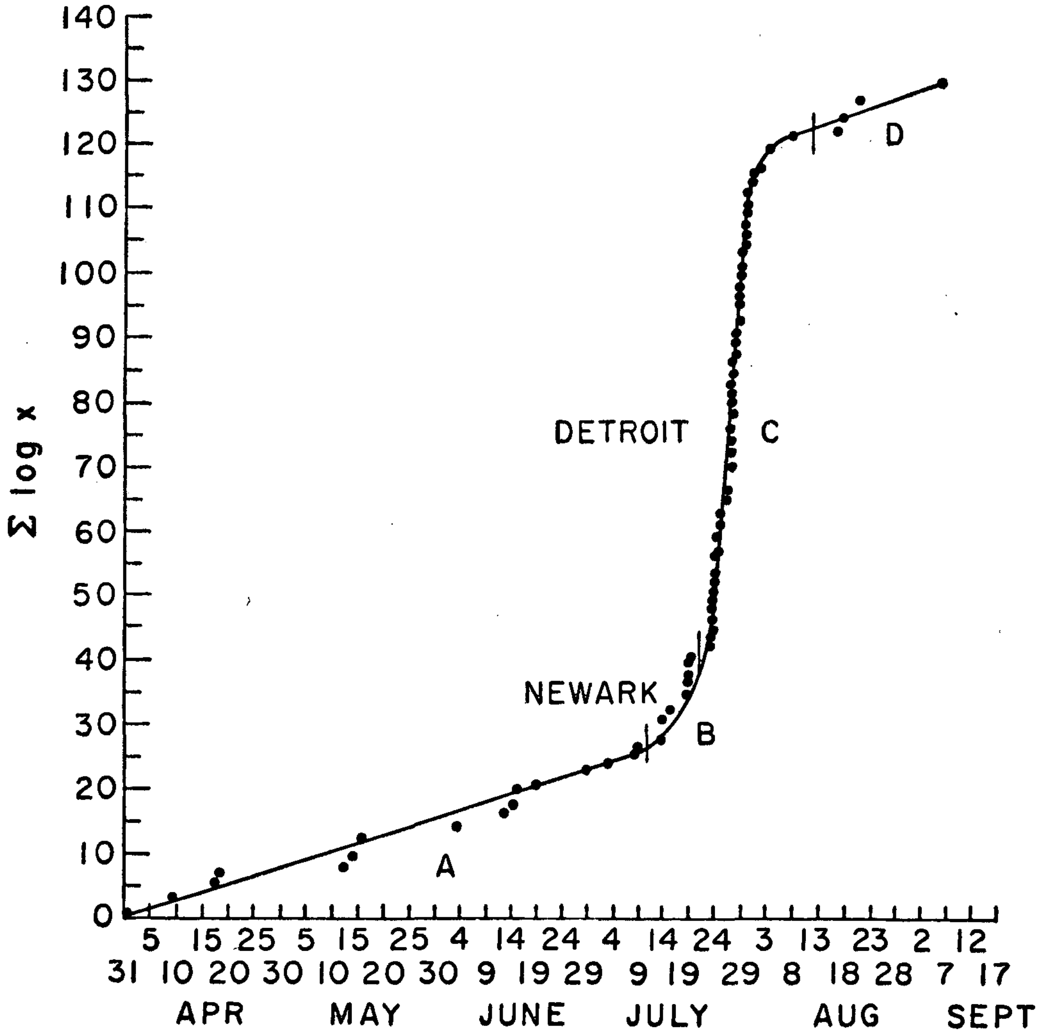

Figure 2

a similar extent.[12] The number of cases in section B is too small to test alone.

The difference between the linear and curvilinear periods in Figure 2 probably resides in the mutual independence of the μ_i. In equation (2), the μ_i were assumed independent from each other. That is, each precipitating incident leading to a disorder, such as an encounter between black residents and the police or shopkeepers on the street in the local community, is independent of all of the others. This is a diffusion process in which each incident results in a consequence similar to other disorders, but each of them began independently of the other. The applicability of the lognormal distribution to 1966 and the first and last portions of 1967 suggests that the assumption of statistical independence was met for these time periods. However, the lesser applicability of this model to the intervening period in 1967 (in particular, the clustering effect shown in Figure 4), indicates that this assumption was violated, and that a different process operated here.[13] This latter period may be termed

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[12] The chi-square value for the C period alone is $\chi^2 = 11.132$, $df = 6$, $.05 < p < .10$.

[13] Other than the hierarchical contagion effect, the only other possible source of difference between the applicability of this distribution for the two time periods is some change in the pattern of arrests by the police in the mid-portion of the 1967 disorder period. However, one would have no reason to expect such change, especially in light of the absence of a contagion effect in 1966 when the Newark and Detroit disorders were absent, and the fact that the disorder frequency leveled off to its prior state in 1967 when these large-scale disorders terminated. In addition, of course, there is the added frequency of disorders especially in smaller cities during this contagion period (a tenfold increase) which cannot be explained by some change in arrest patterns by the police.

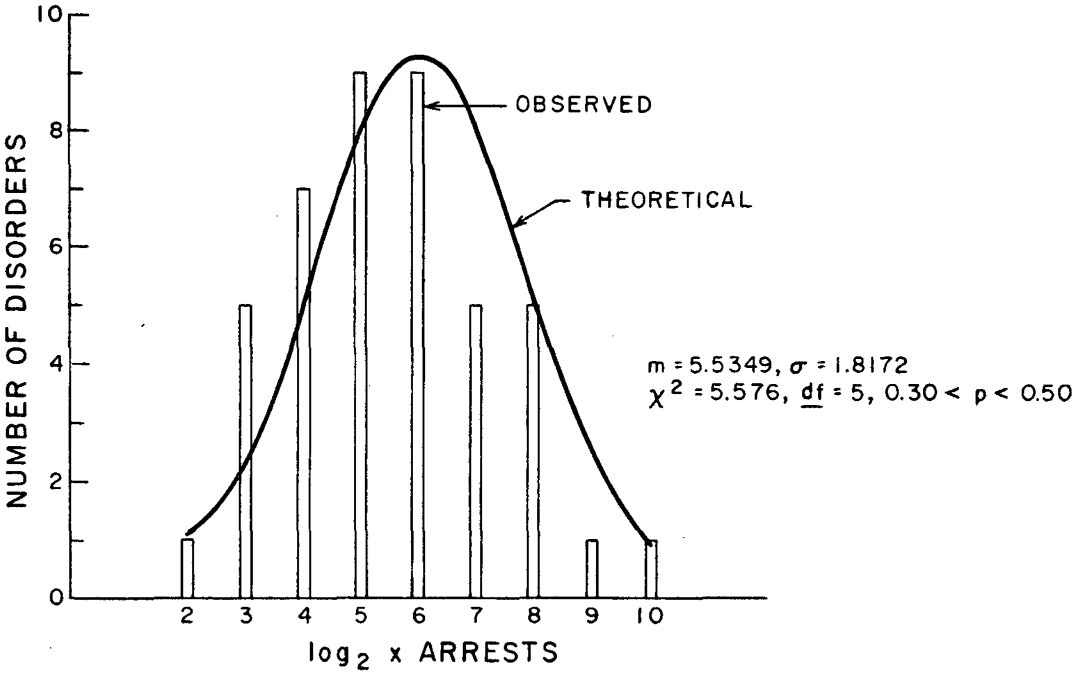

Figure 3

contagious, in which separate incidents are not required to spark a disturbance, but a common process may have operated to generate the disorders.

A Theory of Hierarchical Effect

Substantively, this period of rapid increase is virtually identical with the Newark and Detroit disorders of 1967. The Newark disorder lasted from July 12 through July 16, and the Detroit disturbance began on July 23 and ended on July 31. Figure 2 indicates that the rapidly increasing sections B and C begin on July 14, and end on August 3. Because arrests are shown on Figure 2 for the midpoint of the duration of each disorder, those reported for July 14 began during the Newark disorder, or immediately after its onset.[14] Thus, the disturbances within the period of more rapid increase are associated, at least temporally, with the occurrence of two other disorders of some size and duration.

Note that the Newark disorder, which was considerably less severe than Detroit's (1462 arrests versus 7231, respectively), is temporally associated with a smaller increase in the beginning curvature portion (section B), but that the Detroit disorder is associated with a more rapid increase (section C). The simultaneous oc-

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[14] There is a data point just prior to July 14 but that is the result of taking the midpoint of the duration for a previous disorder. The last section (D) is taken from August 8 because certain disorders from the previous contagion period did not end until that time.

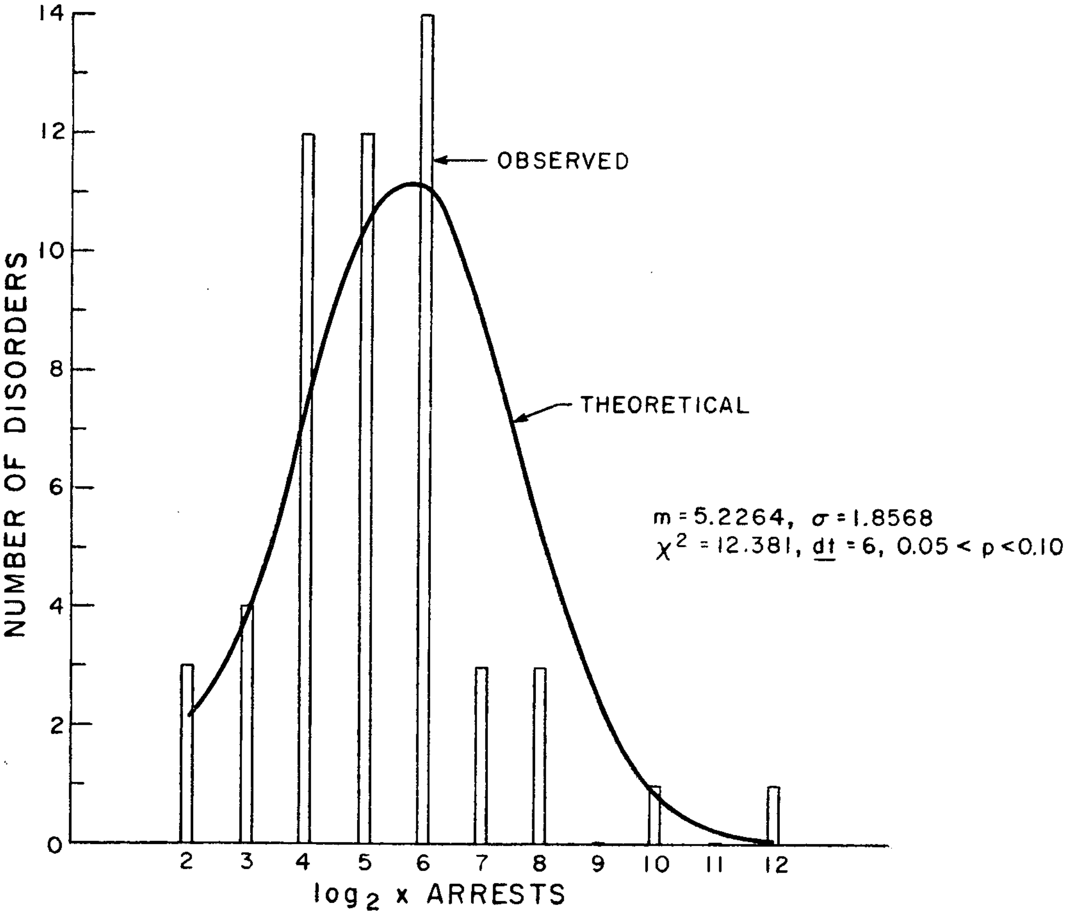

Figure 4

currence of these events in itself, of course, does not directly imply causation of any kind. However, within the context of a theory of contagion, such simultaneity may have considerable significance and, in fact, may be subject to empirical test.

In prior studies of contagion phenomena, the process of adoption generally was found to conform to a hierarchical pattern. The pattern of adopting innovations in the American states, the adoption of social security policy by nation-states (Collier and Messick, 1975), or the patterning of military coups in Latin America all tend to follow a hierarchy of either wealth, modernization, or diplomatic status. For example, if either Argentina or Brazil as a prototype country with high diplomatic status were to experience a coup, then the probability of another Latin American country experiencing a coup within a one-year period is significantly greater than if a country such as Nicaragua or Ecuador with lower status serves as a prototype for instability (Midlarsky, 1970).[15] This hierarchical principle has appeared in a fairly wide variety of contexts.

These patterns in prior findings suggest that something in the nature of a hierarchical process may have operated during the period of the Newark and Detroit disorders, July 12–31, 1967. Both Newark and Detroit are large, visible cities, and the city of Detroit especially would tend to be a prototype for other, generally smaller cities. The Detroit disorder also is associated temporally with the largest increase in the number of disorders. In a city having the fifth largest population among American cities and a large, visible black population, a lengthy and intense disorder might induce contagious effects in other cities. The violence may have rapidly diffused from the larger cities (in this case one of the largest) to the smaller ones, following a hierarchical model.

To test the hypothesis of a hierarchical contagion effect from a large city, we tabulated the number of disorders by city population size for this category. This was done separately (1) for July 14-August 3 (sections B and C of Figure 2), and then (2) for the combined period April 1 to July 12 and August 8 to September 7 (sections A and D of Figure 2). The latter two periods occurred, respectively, before and after the period of maximum contagion (sections B and C), and the similarity in slope and approximation to a straight line suggests that a similar process occurred here, in fact, one similar to that found in 1966 (see Figure 1).

Inspection of the disorder data suggested that cities below some moderate black population size were overrepresented in the disorders of July 14-August 3, in comparison with the preceding and subsequent periods. The cutoff appeared to center at the 40,000 nonwhite population value, and we counted the number of disorders separately for those cities above and below the 40,000 cutoff for the two separate periods. These data are summarized in Table 2. Is the proportion of smaller cities greater in the contagion period than in the noncontagion period as suggested by theory? A chi-square test of significance yields a value of $\chi^2=18.226$, which is significant at $p<.001$ ($df = 1$). Thus, the contagion period demonstrates the hierarchical effect of an increased frequency of disorders in smaller cities when a larger, more visible prototype is experiencing that same phenomenon. The effect is a strong one, as suggested by the magnitude of the chi-square statistic.

To investigate the relative peakedness or flatness of chi-square values in the neighborhood of the 40,000 cutoff, we examined both 30,000 and 50,000 values. For the 30,000 population cutoff, a value of $\chi^2 = 15.229$ ($df = 1$, $p < .001$) was calculated, while the 50,000 figure gave a chi-square value of $\chi^2 = 14.319$ ($df = 1$, $p < .001$). Successive population cutoffs, both smaller and larger than the 30,000 and 50,000 values, provided decreasing values of chi-square. As a result, the 40,000 value provides maximum differentiation, but the maximum is fairly flat and extends in the range of 30,000 to 50,000 persons. This would accord with the expectation that the distinction between large and small city behavior, or the onset of contagion effects, is not a discrete process with threshold values, but rather is one in which there is a continuous increase in the propensity for modeling behavior of large cities by smaller ones as the size of the city decreases.

An Interaction Effect. The interaction between police and black city residents was very likely the precipitating agent for the onset of disorders in large cities but not in smaller ones subject to a contagion effect. Morgan and Clark (1973) argued that the sizable correlation they found between total city population size and disorder frequency was a consequence of the interaction between police and black city residents. However, they did not differentiate among city sizes in their analyses. More direct

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[15] A comparison between the diffusion explanation of instability in Latin America and domestic factors such as industrialization, communication, education, and transportation is found in Midlarsky (1975). These variables did not have an impact of any size or significance on the occurrence of Latin American coups.

Table 2. Frequency of Disorders by Nonwhite Population Size during Contagion and Noncontagion Periods

	Cities under 40,000 Nonwhite	Cities over 40,000 Nonwhite
Contagion (July 14–August 3)	42	11
Noncontagion (April 1–July 12 plus August 8–September 7)	6	16

$\chi^2 = 18.226, df = 1, p < .001.$

evidence for a size effect is found in the differential effects of city size upon disorder frequency in which the larger the city, the greater the degree of association between police force size and disorder frequency (Midlarsky, forthcoming 1978).[16] One can infer from this that the interaction between police and black residents in large cities led to the relatively high degree of variation of total city population size with disorder frequency. The interaction opportunity simply will be greater in larger cities. On the other hand, residents of smaller cities exposed via the communications media to an ongoing major disorder such as Newark or Detroit would not require a separate precipitating incident.

In order to test directly the hypothesis of an interaction effect in large cities, we constructed the interactive variable "police force size multiplied by size of the black population," and found a value of $r = .78$ ($N = 16$, $p < .001$) between this variable and disorder frequency in cities having a black population over 70,000.[17] This same value was found for cities having over 100,000 black population, and exceeded the correlations for all other size variables. Further, in a step-wise multiple regression analysis including this variable and the variables, size of the total population, size of the black population, and size of the police force, no significant variance was added to the equation beyond the first variable included, namely, the interactive effect.

Note that this is the first *direct* specification of an interaction between police and black city residents, the Morgan and Clark results having been inferred from total city population values. This interaction effect was much less for smaller cities below 30,000 nonwhite popularion ($r = .17$, $N = 35$) with the largest correlation with disorder frequency being $r = .20$ for black population size. It would be expected that an endogenous variable would be somewhat more effective in explaining a contagion process in smaller cities, although we found neither this nor any other variable statistically significant, suggesting that a contagious process is much less reliant on specific causal patterns of association. Neither the additional presence of the police nor a larger black population size should significantly add to the disorder probability when that probability is dependent on a rapid contagious process. Simply the presence of an identifying population of some minimal size plus the behavior of the model in the larger city should be sufficient for the contagion effect to take hold.

Conclusion

The use of the lognormal model has resulted in a distinction between two types of diffusion processes. One is the diffusion of disorders in which each precipitating incident in the form of an interaction between police and black city residents in large cities is independent of others, but results in a similar outcome proportionate to previous disorders. Here the lognormal distribution is clearly applicable, and was found to apply to several intervals in this time period. The second process consists of the dependence of small cities upon the behavior of large ones, and the accelerated occurrence of disorders in small cities when larger ones are experiencing disturbances. This form of direct contagion is hierarchical and was found to operate in the period coterminous with the Newark and Detroit disorders. The approximate value of 40,000 black population size differentiates between behaviors in the two city size categories.

In portions of this study, the term *diffusion* has been used interchangeably with *contagion*, or indeed has been used as the generic category for the spread of certain behaviors as has been done in prior analyses. Given the findings of this study, it is appropriate now to explicitly differentiate between the two phenomena.

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[16] Morgan and Clark (1973) found a correlation of $r = .59$ between total population size and disorder frequency and in addition found that the variation of this variable with disorder frequency was due almost entirely to size of the police force and black population size, thus implying the interaction effect between the two variables. I found (Midlarsky, forthcoming 1978) that for cities over 70,000 black population size, police force size was equal to total population size in explanatory power ($r = .77$) and substantially greater than either black population for this city size or police force size for smaller cities.

[17] Values for nonwhite population and total population size are found in the 1960 census values for cities over 50,000 total population size, while the police force values are taken from *The Municipal Yearbook* (1967).

Since the assumption of independence is originally incorporated in the Kolmogorov diffusion equation from which the lognormal distribution is derived, we should properly maintain continuity with this tradition. Thus, diffusion can be defined as the spread of a particular type of behavior through time and space as the result of the cumulative impact of a set of statistically independent events. The modal response to the independent precipitating events may be the same (a disorder), but the individual initiations are mutually independent. Contagion as the direct modeling process, on the other hand, can be defined as the spread of a particular type of behavior through time and space as the result of a prototype or model performing the behavior and either facilitating that behavior in the observer or reducing the observer's inhibitions against performing that same behavior.[18] Clearly, the second process is more susceptible to social-psychological investigation of modeling behavior than is the former which relies on sui generis processes which cumulate over time. On the other hand, whether these distinctions will continue to have theoretical and empirical utility can only be answered by future research.

This study demonstrates the need for theory and model construction in the analysis of diffusion and contagion phenomena. Spilerman (1970, 1971) simply used the negative binomial as an empirical measure of overdispersion (compared to the randomness implied by the Poisson), and also used empirical curve-fitting techniques to fit black population size to an S-shaped function in relation to the disorders. The lognormal distribution as a statistical model, on the other hand, emerges out of proportionate effect theory and the hierarchical contagion hypothesis was confirmed by the data, Spilerman having investigated only a geographic contagion and rejecting that alternative. The hierarchical contagion effect is a strong one, as shown in Table 2 and its associated chi-square value. The increase in the number of disorders per unit time (a tenfold increase) when the larger disturbances are ongoing is another strong manifestation of this process.

This research emphasizes the importance of longitudinal analyses along the lines of Figures 1 and 2. Without such analyses, we would not have noted the increased frequency of disorders in mid-1967, and might have overlooked the entire hierarchical mechanism emanating from prototype behavior in larger cities to smaller ones. This apparently is what happened in Spilerman's cross-sectional analysis of the disturbances. Disorders occurring in 1961 and 1962 were aggregated together with disorders which took place in 1966 and 1967 without regard for the processes which occurred over time and which gave very different properties to the later disorders, especially those occurring in smaller cities.

In assessing the implications of these results for the decade of the 1970s, we should bear in mind that the climate today in large cities is different from that of a decade ago. Blacks now have a somewhat greater share of political offices in the urban setting, and also have come to occupy a somewhat larger proportion of police jobs. Thus, the authority structure may seem less alien and the police a less abrasive force both as individuals and as representatives of the existing authority structure. However, if such gains in political access commensurate with numerical proportions of urban populations do not continue, then the environment could once again become more alien and abrasive than it is today, with possibilities for new diffusion and contagion processes along the lines of the past decade.

A quite specific implication of this study concerns the form and time dimensions of diffusion and contagion processes. Initially, the disorders would likely follow a diffusion process caused by some interaction between a minority population and elements of its environment. The transition to a contagious process would be slow as is shown in Figure 2 where the transition from period A through B to C in fact takes several weeks.[19] In addition, a salient event such as a lengthy disorder in a large visible city like Newark or Detroit, would be required. Thus, the infrequent summer disorders which have occurred periodically in cities during the 1970s are not likely in themselves to result in a major contagious process. However, if there were to be an extended disturbance in a large, visible city then it could result in the fairly widespread contagion of disorders throughout the country. One way of minimizing the probability of such processes occurring is to minimize the possibility of precipitating events leading to disorders in large cities. The role of the police or other representatives of authority structures in large cities then is essential and requires minority representation both at the individual level of the police officer or shopkeeper, and at the higher levels of political authority structures.

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[18] For a review of the several meanings of *contagion, imitation* and related concepts, see Wheeler (1968). It is possible that during the contagion period the modeling effect occurs as the result of a perceptual transformation of certain routine events such as the arrest of a black person by a white police officer. This event then becomes the basis for a disorder. In this instance, of course, any such event would serve the same purpose because of the predisposition of the population, and so this circumstance is functionally equivalent to the lack of any unique precipitating event.

[19] Two additional features of the analysis concern the timing of the disorders. First, as seen in Figure 2, the transition from the diffusion to the contagion process is rather slow (from A to B and C), but later the contagion period (from C to D) ends somewhat abruptly. The initial sluggishness of the process may be due to certain inhibitory mechanisms as the result of socialization to nonviolent norms which may prevent individuals from rapidly participating in a disorder. However, once the modeling effect is removed at the termination of the two major disorders,

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then the inhibitory mechanisms appear to quickly reassert themselves. Second, although there are clear differentiations between the diffusion and contagion processes in Figure 2 and in the chi-square analysis of Table 2, still both processes may exist simultaneously to a limited extent. This is reflected in the less than perfect fit of the lognormal distribution in Figure 3 plus the fact that when the lognormal is not applicable as in Table 1 or Figure 4, the degree of lack of fit is not especially severe. This would be expected given the closeness and probable overlap in timing of the two processes.

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COMMUNICATIONS

TO THE EDITOR:

I am afraid I am among those who have in recent years found the *APSR* to become depressingly unreadable, as arcane notations replace expository English and political insight yields to statistically demonstrated trivia. But I am delighted to discover that *APSR* has after all a saving sense of humor. I refer to your selection of perhaps the nation's most unregenerate right-wing political scientist to review my book *In Search of American Foreign Policy: The Humane Use of Power* (Vol. 71, December 1977).

That a broad reevaluation of foreign affairs liberalism should be entrusted to a dedicated opponent of liberalism in any form was a deliciously witty act, ensuring as it did that the entire effort would be ridiculed rather than critiqued, and that it would be tested entirely in terms of its deviations from true orthodoxy regarding the Soviet Union as revealed by Professor Possony.

I can envisage a regular humor section in the "Book Reviews," in which, to give only a few obvious examples (to which I am by no means comparing myself), books by Carter are reviewed by Reagan, Woodward-Bernstein by Nixon, Gandhi by Nietzsche, Rathjens by Wohlstetter, Tizard by Lindemann, Galileo by Torqemada, etc., etc. Prizes might be offered for the funniest mismatchings, thus converting to a delightful game the otherwise heavy weather of serious and informative book reviewing.

Of course, since the review in question appeared four years after publication, it (like all *APSR* reviews) is essentially useless to the profession, and the matter is really moot. For that reason I will restrict myself to observing that the far Right's resort to contempt and ridicule when confronted with anti-Sovietism that is insufficiently dogmatic, bears an uncanny resemblance to the outrage on the part of true Stalinists in the face of familial heresies such as democratic socialism and Eurocommunism.

Keep up the good work. Pretty soon the profession will become totally invisible—all clothes, as it were, but no emperor.

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TO THE EDITOR:

I was dismayed to read Leonard Binder's harsh review of Itamar Rabinovich's fine book *Syria Under the Ba'th*, 1963–66 (*American Political Science Review*, March 1976, 254–55). I believe he has missed the point.

Binder starts out his review announcing his own opinion that "the central question of Syrian politics is how to explain the evidently stable position of the Ba'th regime. . ." and then castigates Rabinovich for his failure to provide such an explanation. "The author knows what it is he should be explaining," he states, "but he apparently believed that a recounting of how one event led to another was a sufficient explanation. . . . To show the linkages between conspiratorial stroke and counter-stroke is a work neither of scholarship nor of analysis."

But there is no reason to imagine that Mr. Rabinovich shares Binder's view of what his job should be. Any why should he? As a historian, he has tried to show what actually happened; and if anything, he demonstrates that what Binder took to be stability in Syria in the 1960s was nothing of the kind, for the regime was a shifting coalition of factions. Rabinovich explains painstakingly how Syria drifted toward the coup d'état of 1966 and the rule of a leftist, Alawite, military-dominated regime, not as a mindless exercise in "hyperfactualism" but in order to reconstruct the inner workings of the political process and provide clues as to why people belonging to the same organizations and professing similar beliefs fell so readily into internecine conflict; or, conversely, that alleged ideological struggles may have concealed much more personal differences.

As even the bare facts of recent Syrian political history are the object of so much ignorance among us, it is perhaps a little early for anyone to be reaching for grand theory, in the guise of an "explanatory hypothesis," in discussing that country. It is precisely the interconnections of events, conspiratorial and otherwise, that we need to learn more about first. Surely any careful reader of Rabinovich's book should be able to understand that making sense of the nonsense in Syria in the 1960s was no simple task, but was an exacting job of scholarship and analysis. Perhaps Binder is able to function as a specialist in Middle East politics without reading authoritative political histories, but for the rest of us in the field, this is bread and butter.

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[Editor's note: publication of Professor Kerr's communication, originally received in August, 1976, was inadvertently delayed.]

1976 Update

Editor's Note: Unfortunately Table 2 of Professor Axelrod's communication (*APSR*, June, 1978, pp. 622–24) was inadvertently omitted. It is printed on the facing page. Also, in Table 1, the plus sign found at the top of the table between columns 24 and 25 should have been a division sign, and in column 11, opposite 1960, the correct figure is 34, not 23. We apologize to the author for these oversights.

Table 2. The Republican Coalition, 1952–1976

	Percentage Contribution						= (Size						x Turnout						x Loyalty)						÷ (NT x NL)	
Year	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NT	NL
1952	75	99	79	75	87	84	64	90	73	74	72	84	72	67	61	58	73	61	56	57	61	61	57	57	63	55.1
1956	84	98	78	75	84	89	75	91	74	75	71	86	67	64	58	56	69	60	59	59	63	62	60	60	60	57.4
1960	83	97	84	90	75	90	77	90	75	75	66	87	70	68	65	61	71	63	50	51	55	63	50	52	64	49.5
1964	89	100	87	80	76	91	81	89	77	74	72	88	67	66	61	60	68	63	40	42	45	44	38	40	63	38.5
1968	90	99	81	80	80	92	84	89	76	74	69	90	65	63	62	60	66	62	44	47	46	49	47	45	62	43.4
1972	93	98	77	70	73	95	88	89	75	69	66	92	58	57	55	53	62	55	61	66	63	65	60	63	56	60.7
1976	97	99	80	76	67	98	71	89	77	70	68	92	56	56	52	54	53	54	49	52	52	53	49	49	54	48.0
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

Year	Percentage Deviation in Loyalty to Republicans					
1952	+1	+2	+6	+6	+6	+2
1956	+1	+1	+5	+4	+2	+2
1960	0	+1	+5	+13	0	+2
1964	+1	+3	+6	+5	−1	+1
1968	0	+3	+2	+5	+3	+1
1972	+1	+5	+3	+4	−1	+2
1976	+1	+5	+4	+5	+1	+1
Column	27	28	29	30	31	32

NP	nonpoor (income over $3,000/yr.)
W	White
NU	nonunion
P	Protestant
N	Northern (excluding border states)
NCC	not in central cities of 12 largest metropolitan areas
NT	National turnout
NL	National loyalty to Republicans

The Political Economy of Charles E. Lindblom

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The consistent theme in Charles E. Lindblom's work is a vision of political economy as constitutional engineering. Lindblom sees the question of institutional design in terms of a mechanical metaphor in which political economic systems are contrived out of relatively simple components. Politics and Markets *compares a broad range of capitalist and socialist systems as a means of evaluating market mechanisms and authority structures as instruments of social coordination and control. Lindblom's argument that the privileged power of the corporation poses a problem for liberal market-oriented societies is logically distinct from his case that the corporation fits "oddly" with democratic theory, and the latter may be the more significant theme for further inquiry in political economic theory.*

The occasion for this essay is the publication of Charles E. Lindblom's *Politics and Markets* (1977). This has been billed as an important book and I intend to treat it as such. I would also use the opportunity to place Lindblom's contribution to contemporary political theory in a certain perspective. This would be necessary to an adequate interpretation of *Politics and Markets* in any event for it is my impression that Lindblom's distinctive purposes as a political theorist have not been very well understood in the discipline. Perhaps his approach has been too original, or perhaps too classic to have strongly influenced the course of political inquiry in this generation. In any event, despite his prominence, Lindblom seems more often cited than seriously argued.[1]

Lindblom identifies as a political economist. To appreciate his work it is essential to understand his particular conception of political economy. The term is once again in vogue in the discipline, and many different approaches to political inquiry have claimed it, with grandly confusing results. To some, political economy means using the techniques of economic analysis to give an account of political phenomena, as in the writings of Mitchell, Downs, Olson, Froelich, Oppenheimer, etc. To others, political economy is the effort to explain economic phenomena in political terms, as in neo-Marxist analysis. Still others think of political economy as what political scientists do when they study economic institutions and policies.

Lindblom's idea of political economy lies closer to the classic tradition, with its roots in eighteenth- and nineteenth-century liberal philosophy. To be sure, this implies a substantive interest in the interpenetration of political and economic institutions and processes. For Lindblom, both politics and economics are truncated disciplines unless each deals with the subject matter of the other. More important, however, is the distinctive function of theory in this conception of political economy. The basic object is not that of accounting for the incidence or development of different types of political economic systems. Rather, the purpose of theory is to create a framework that would permit a prudential calculus of desirable political economic institutions and policies. In this mode of analysis, political economic structure is seen as a problem of deliberate and reasoned public appraisal and choice rather than as a phenomenon to be explained.

The greater part of Lindblom's mature work can be seen as a sustained investigation of the properties of certain basic processes of social decision, coordination and control, such as hierarchy and authority, bargaining and market mechanisms. *Politics and Markets* lies in a clear line of descent from *Politics, Economics and Welfare* and *The Intelligence of Democracy*. To a large extent, it is a simplification, clarification and extension of these earlier works.

The mechanical metaphor underlying Lindblom's thought is particularly significant. The complex systems of planning and control that characterize modern society can be visualized as "constructed" out of relatively elemental components. All systems are "built up" as combinations of these elements and they can be defined and classified according to the patterns that these combinations take.

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[1] In a recent essay on the work of Lindblom's colleague and collaborator Robert A. Dahl, George von der Muhll (1977) noted that Dahl and Lindblom's *Politics, Economics and Welfare* (1976) was "more often footnoted than closely read within the discipline." The comment might also be made of David Braybrooke and Charles E. Lindblom, *A Strategy of Decision* (1963) or Lindblom's *The Intelligence of Democracy* (1965).

However, the mechanical metaphor is not intended merely to capture the dynamics of complex political economic order. It has a second, more significant function. The basic processes appear as a "kit of tools" in the hands of the policy maker. One *designs* processes of decision, coordination and control on the basis of authority structures, market mechanisms and democratic processes. The basic elements are the "machinery" that can be used, creatively adapted by the political engineer, for the resolution of public problems.

Once this fact is recognized, the audacity of Lindblom's approach as well as its affinity to the classic tradition of political economy becomes fully apparent. To an age accustomed to thinking of social structures as the product of historic circumstance and development, he is suggesting that we treat the constitutional question in the largest sense, that of the desirable arrangement of political and economic institutions, as though it were a matter for considered public choice.

Politics and Markets

It is against this background that *Politics and Markets* should be read. Many view this book as a sustained case against the power of the business corporation in liberal democratic societies. However, *Politics and Markets* does not really take the form of an argument. Rather, Lindblom seems to write as the policy advisor preparing a staff paper for a civilization. He is giving industrial society its options. He sets forth a logically coherent set of alternatives for using authority structures and market mechanisms as instruments of public action, appraises the costs and benefits of each in terms of such liberal values as popular control, efficiency, liberty and equality, and examines how various combinations of these instruments work out in practice both in market-oriented and socialist systems. In the end, the analysis culminates not as an argument for a preferred solution but as a statement of trade-offs.[2]

Nonetheless, the structure Lindblom imposes on his analysis decisively shapes the outcome. The four basic processes of social coordination and control that were presented in *Politics, Economics and Welfare* (hierarchy, the price system, bargaining and polyarchy) are collapsed into two: authority and the market. (Lindblom does introduce, somewhat tentatively, a third process, "persuasion," but the theme is not fully developed.) The consequence is that *Politics and Markets* presents some starker contrasts than *Politics, Economics and Welfare.* Either we plan through authority structures or market mechanisms, and if through authority structures, then we must have regard for democratic rights and processes, else we face a problem of freedom and popular control. The scheme seems to do less than justice to certain obvious and important options for political economic institution building. For example, the characteristic neocorporatist mechanisms whereby important functional organizations are brought into structured collaboration with economic policy making—such as French indicative planning or Swedish national wage bargaining—are given only passing attention. It is not that Lindblom regards such innovations as unpromising or insignificant. It is rather that his scheme cannot focus attention on them—as Andrew Shonfield did—as a potential third class of instruments for the design of political economic institutions.

The book opens with a discussion of the basic elements: state authority, the "political" facet of political economy, and the market, its economic aspect. The advantages and disadvantages of each are surveyed. Market mechanisms are efficient allocators of resources and instruments of social coordination that are uniquely compatible with individual freedom. However, they are subject to the conventional catalogue of "market failures." Authority systems can be highly effective but they are also imprecise, clumsy and often irrational. They have "strong thumbs, no fingers." One would not want a pure market or authority system Lindblom, always the protagonist of the mixed economy, suggests. Furthermore, the two mechanisms always appear in intricate combinations in practice.

Almost all socialist systems contain strong market elements; in fact it is really only the market for transactions among firms that is planned in socialist systems. Labor markets and consumer markets generally remain, at least in some form. Similarly, all market-oriented systems engage in some degree of central planning. In fact, the market can be used as an instrument of state authority and control. One can distinguish between the familiar "consumer sovereignty" market form, in which production responds to individual preferences, and "planner sovereignty" systems, in which production responds to the desires of policy makers. In fact, all modern capitalist systems practice some degree of planner sovereignty. Keynesian mechanisms of economic management are, for example, largely based on the way governments decide to act as economic units, how they consume and how they derive revenue from the society.

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[2] The approach is in many ways similar to that adopted by Arthur Okun in *Equality and Efficiency: The Big Tradeoff* (1975). The genre itself is perhaps more characteristic of economics than political science.

Lindblom proceeds to a consideration of markets and polyarchy as means of control over political authorities. This leads to a comparative examination and evaluation of private enterprise and socialist systems. The discussion is fair, judicious, balanced and—on the whole—thoroughly conventional. Some capitalist nations have achieved more equality than any European communist system. However, the family of capitalist systems is less egalitarian on the whole than socialist ones. Central planning proves less efficient than markets as a mechanism for stimulating productivity and technological innovation.

Much of what Lindblom has to say is provocative and original and much of it is familiar. Large portions of conventional wisdom are simply embedded in the argument without much reflection or criticism. The review of market failures offered in chapter 6 could have been drawn from any beginning economics text. The discussion of business power detailed in chapter 14 reads very much like a revisionist American government textbook of the 1960s. The entire consideration of communist planning would be suitable for an introductory comparative politics course. In many ways, this characteristic of the book is an advantage; *Politics and Markets* will no doubt have wide classroom use for years to come. However, the result is that Lindblom does not advance the argument as far, as forcefully, or as plausibly as he might have. As a review and synthesis of the commonplace arguments for and against planning and the market, modern capitalism and modern socialism, it is superb, but it often does not transcend the established terms of debate.

The Distinctive Power of the Corporation

Whatever the contribution of *Politics and Markets* to the theory of political economy, discussion of this book will inevitably focus on Lindblom's provocative view of the distinctive power of the business corporation and his second, somewhat different, case that the corporation does not "fit" with democratic theory. Inevitably too, much of what is said about this thesis will simply reflect established partisan affinities. Lindblom is apt to be applauded as an "establishment liberal" who "saw the light" or condemned for a scurrilous attack on the business community. For those who hold no standing general brief either for or against the dominant institutions of modern capitalism, the question of course is not where Lindblom stands but whether what he says makes sense. The issue is really whether the power of the corporation is as Lindblom represents it and whether it is unique.

Lindblom begins with an analysis of the "public function" of the corporation. The corporation is not merely an economic actor. It partakes of functions that are characteristic of political authority. Corporation leaders make what are in effect planning decisions about the direction and distribution of production. The authority of the corporation does not derive from the normal processes and institutions of polyarchy and is hence problematic for democracy. Furthermore, a distinctive characteristic of private enterprise market systems is that corporate leaders must be induced to perform their public function. Government cannot simply command business executives. They must offer special benefits to them in order to secure their satisfactory performance (pp. 172–75).

So far so good. Lindblom's views may not be uncontroversial but they are not, I should think, problematic. It is the next move in the argument that is more perplexing. Lindblom tries to support his claim that the power of business managers is unique and in some ways all-compelling by means of a kind of latter-day power elite analysis. The power of business rests on resources of money, organization and access which distinguish it from all other interest groups. Business also has the capacity to set the policy agenda and remove its own privileged position from political debate through social indoctrination, largely accomplished, it would seem, by flooding the schools and universities with instructional materials favorable to the free enterprise system.

Lindblom's selective perception of reality is familiar but it is not particularly convincing to those who have covered this ground before. It raises all the inevitable issues about potential and realizable power, about "false consciousness" and the like. In this section, Lindblom seems to lose the characteristic cool, judicious balance that is so evident in the rest of the work. There is no place in his analysis for the evidence of increasing public, intellectual and press distrust of business leadership, or for the perspective of a business community that sees itself increasingly on the defensive, faced by adversary as well as cooperative public bureaucracies and regulatory agencies, challenged at many turns by a new array of environmental, consumer and populist groups and forces. The fact that Americans do not seriously entertain the socialist alternative is hardly convincing evidence of capitalist indoctrination. It may be that modern industrial societies are no more convinced by one arcane formulation of the political economic problem than they are by another.

Perhaps Lindblom's case would have been more compelling had he taken the thesis of capitalist hegemony in full seriousness, had he evoked Gramsci, Habermas, Miliband and Offe rather than the more pedestrian arguments of revisionist American political science of the 1960s. The heroes of *Politics and Markets*, Lindblom says, are Adam Smith and Karl Marx—Smith for his appreciation of the powers of the market mechanism, Marx for his insight into the political implications of the institutions of industrial society. Yet Lindblom never really does justice to the force of neo-Marxist analysis.

There are further difficulties with Lindblom's thesis. His claim is that corporate power is distinctive and its public role unique. The labor union does not perform a public role like the corporation but rather has a purely private, or "factional" function. This is certainly an arguable proposition. The capacity of organized labor to sustain or to thwart macroeconomic policy is a staple of contemporary political economic commentary. The idea that the trade union does not play a crucial role in public planning and policy making would certainly seem surprising to a Scandinavian or a Briton.

It is interesting to note that earlier in his career Lindblom made a case quite similar to the present one for the distinctive power of another political economic institution, and on that occasion, it was precisely the trade union that was the cause of his concern. In 1949, Lindblom saw a shift of power in the direction of big labor, on the grounds of its unique political advantages. Union leaders would be pushed by their followers to interfere with the market, creating rigidities and inflation. In the competitive struggle, union leaders had the advantage of an egalitarian philosophy that ran stronger in liberal ideology than the rights of property that legitimate corporate capitalism. The industrial monopolist faced many problems in controlling competitors, regulators and substitute commodities, but the union need not control the supply of labor but only the buyer of labor, who has every incentive to reach agreement through collective bargaining (Lindblom, 1949). The structure of this argument has fascinating parallels with that advanced in *Politics and Markets.* A plausible case is made that a specific institution possesses distinctive political resources that pose a threat to the integrity of a market-oriented, liberal economy. It is interesting to note that Lindblom's present point is that the threat he anticipated almost 30 years ago never materialized.

My point is not that corporate power is not a serious concern for liberal democracy but that Lindblom's portrayal of it would not advance the argument in the eyes of an observer as yet uncommitted to one of the standard orthodoxies. The case is overdrawn. As one who once found in *Politics, Economics and Welfare* a fascinating avenue of escape from the rigidities of the debate on public planning, I am sorry that the argument has been relaid in its venerable procrustean bed.

The Corporation and Democratic Theory

Politics and Markets ends on a puzzling and enigmatic note. "The large corporation fits oddly into democratic theory and vision," Lindblom writes, "Indeed, it does not fit." Earlier he argues, "Clearly democratic theory needs to be extended to take account of what we will call the privileged position of business" (pp. 5, 356).

Now this is not precisely the same case as that which has been made about the distinctive power of the corporation. It is one thing to say that the corporation poses a problem for democratic *practice* and another to say that it poses a problem for democratic *theory*. What is at issue now is not the power of the corporation but how we appraise it. If Lindblom is indeed making this point, then he is raising a far more interesting issue than the claim that the corporation is incompatible with democracy. For it might be that we have been asking the wrong questions and appraising the political economic role of the corporation on the wrong grounds, in the light of inadequate standards of judgment.

If that is the case, then the question we must put is clear. What would render corporate power legitimate? What standards are appropriate for judging the public performance of the business corporation in a democratic society? It is a problem that has been addressed by Willard Hurst (1970) and Grant McConnell (1966), among others. McConnell puts the issue nicely: "The persistence and growth of private power has posed an embarrassing problem for all who are involved in exercising it. The problem is authority. What justifies the existence of power; by what principle is it rightful? For if it is not justifiable, power is properly open to attack and, if possible, destruction" (McConnell, 1966, pp. 51–52).

The answer to this question is not obvious. To argue for industrial democracy, for example, on the ground that the corporation is *like* a government might be a misleading analogy. (It was Aristotle, after all, who suggested that the governance of each institution posed a distinctive problem.) Further, as Lindblom's colleague Robert Dahl has frequently observed, the idea of industrial democracy itself poses some knotty problems for democratic theory. What indeed is the constituency of the corporation? How guarantee representativeness in such an institution?

The fact is that liberal democratic theory sets very demanding tests for the legitimacy of any political economic institution and for the corporation in particular. The ideal of the market is a strenuous normative principle indeed. It must be shown that in all significant respects buyer and seller are contractually equal parties, else the validity of the relationship is subject to legitimate criticism. In similar fashion, any departure from equality of voice and vote, any concentration or aggregation of power, is problematic for the kind of democratic theory espoused by Dahl and Lindblom. Taken in this strict sense the evaluative principles of liberal economics and democratic theory entail an almost utopian theory of legitimacy. They are in fact of greater power for those who would engage in social criticism than for those who would attempt to justify any extant set of social institutions.

If the corporation "fits oddly" with democratic theory, the problem may not be only with the corporation but also with the theory. It may be that a version of democratic theory that has no sure, consistent category for institutions as it does for individuals cannot quite do justice to the appraisal of the civilization that has been created under its auspices. Socialists, neo-corporatists, Christian Democrats, American populists and pluralists have all suspected as much. Perhaps we do need to begin to think again beyond the universal categories of classic liberalism to a more differentiated, perhaps more organic, conception of the democratic political economy.

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BOOK REVIEWS

Political Theory and Methodology

Structure of Decision: The Cognitive Maps of Political Elites. Edited by Robert Axelrod. (Princeton, N.J.: Princeton University Press, 1976. Pp. xvi + 404. $25.00, cloth; $9.95, paper.)

What are a political decision maker's beliefs? How do such beliefs influence what happens in the decision-making process? Scholars from many disciplines have long been interested in the beliefs of political elites. This work contributes to research into the beliefs of political elites by describing several ways of determining the beliefs of political decision makers.

Robert Axelrod and his contributors focus on defining a decision maker's (or collectivity's) cognitive map of an issue. What are the individual's or group's goals and what are the causal linkages among these goals or objects? By ascertaining causal assertions about goals or objects from decision makers' statements or judges' ratings, one can graph a policy maker's (or group's) belief system on the issue. This graph Axelrod and his contributors call a cognitive map, which captures "the structure of the causal assertions of a person with respect to a particular policy domain," and generates "the consequences that follow from this structure" (p. 58).

Following two introductory chapters by Axelrod on cognitive mapping as an approach to analyzing decisions and Ole Holsti's insightful review of research on the role of cognitive processes in foreign policy formation, Axelrod discusses five studies using cognitive maps which show how such maps can be derived and used. Axelrod has constructed cognitive maps for each of the principal participants in the deliberations over Persia of the British Eastern Committee in 1918, building them from verbatim transcripts of these meetings. Stuart Ross describes the cognitive map of Gouverneur Morris who was quite influential at the American Constitutional Convention in 1787. The cognitive map is based on James Madison's notes of Morris' speeches at the convention. G. Matthew Bonham and Michael Shapiro examine the cognitive map of an advisor on the Middle East to high-level U.S. officials who had participated in a simulation of a Jordanian crisis just prior to the actual Black September crisis in Jordan in 1970. Basing their predictions on the specialist's cognitive map from the simulated crisis, Bonham and Shapiro anticipated his responses to the Black September events. They compared their predictions to interview responses with the specialist following the actual Jordanian crisis. Using experts from Rand's NSF Energy Project team, Fred Roberts mapped the variables and causal connections the experts perceived important in the demands for energy by the transportation sector. Roberts was interested in what happens if one of the variables is increased or decreased. Also relying on experts, Jeffrey Hart compared and contrasted information from experts' cognitive maps of the acting nations involved in the Law of the Sea Conference in Caracas, Venezuela in June 1974.

The five studies exhibit the varied ways of using cognitive maps. Such maps can be derived for single policy makers or for collectivities. Cognitive maps can be used to prescribe (suggest what needs to be changed to get different and better decisions) and to describe (indicate what the belief system of a particular decision maker is). They can be used for forecasting (if we increase or decrease certain variables what is expected to happen), for explanation (why did we get the change in decision making we found based on the decision maker's cognitive map), and for decision making (given specific goals which policies will be accepted and which rejected). Axelrod discusses these various uses along with the limitations of cognitive mapping and research projects that should be undertaken in the last three chapters of the book.

Axelrod and his contributors present an intriguing way of examining political decision makers' beliefs and cognitive processes and at first blush this seems a way that merits more research attention. Because Axelrod's cognitive mapping technique is in the formative stages, however, it seems appropriate to raise several concerns for future researchers to consider. (1) An examination of the coding rules suggests that rather extensive training will be necessary to learn to derive a cognitive map. Coding rules appear in the appendices of the book. (2) As defined, a cognitive map focuses on a specific problem (issue, decision, policy domain). Is there any generality to the cognitive map or must one have a replication of the particular problem for the cognitive map to be applicable again? The simulation of policy makers' belief systems that Bonham and Shapiro have designed appears more general than the cognitive maps presented elsewhere in the book. (3) How stable are cognitive maps? Given a slight change in the situation, another set of experts, another type of document, do the maps change? In addition to inter-coder and inter-judge reliability, it is important to consider the reliability of cognitive maps with changes in material, experts, and situations. (4) Are some political decision makers more likely to have a stable cognitive map than others? Some policy makers seem open to new information, changing their causal assertions as a result of new data from the environment. Other policy makers fit environmental information to their views of the world, changing or selectively filtering new data to match their belief systems. (5) Axelrod assumes that decision makers feel constrained to act consistently with their public statements and, in turn, their cognitive maps. This assumption does not take into account such factors as situational constraints, delegate status, resource constraints, political pressures. Particularly in decisions made by groups, what one decision maker decides may not be congruent with what is possible at the moment or with the decisions of the others in the group. Hart has begun to explore the effects of some of these constraints in his examination of the cognitive maps of the Law of the Sea conference.

Holsti has commented: "The landscape of cognitive studies is littered with possibly useful 'models' that have failed to generate empirical follow-up studies" (p. 36). With the advent of this book by Axelrod, such seems an unlikely occurrence for the cognitive mapping of the belief systems of political elites.

MARGARET G. HERMANN

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Power and Political Theory: Some European Perspectives. Edited by Brian Barry. (New York and London: Wiley, 1976. Pp. xvi + 296. $18.95.)

To facilitate an exchange of ideas among political scientists in various European countries, the European Consortium for Political Research (ECPR) asked Brian Barry (then at Oxford) to organize a workshop on political theory, which was held at the University of Strasbourg in April 1974. This book consists of 12 of the 28 papers presented at that workshop (all substantially revised), plus an original concluding essay.

The principal concern of the volume is the nature of power, to which the first seven papers are addressed. The first two—"The Limits of Exchange Theory" by Jack Lively, and "Power Divorced from its Sources" by Pierre Birnbaum—examine Peter Blau's exchange theory of power and find it basically inadequate for political science because of its failure to deal with structural and distributional aspects of power. The other five papers on power—"Power and Influence as Political Phenomena" by R. J. Mokken and F. N. Stokman, "Power, Cause and Force" by F. Chazel, "Power: An Economic Analysis" by Brian Barry, "Power and Causation" by Felix Oppenheim, and "Cooperative Negotiations and Bargaining" by Knut Midgaard—explore a variety of persistent issues in the conceptualization of this phenomenon and present contrasting approaches to the analysis of power processes.

The remaining five workshop papers in the book deal with diverse problems of political theory. Franz Lehner and Hans Gerd Schütte, in "The Economic Theory of Politics," argue that economic theory can explain the workings of political systems provided relevant models are employed. In an essay titled "On Some Problems of Political Theory," Johan K. De Vree raises a number of philosophical questions concerning the linkages between individual behavior and political processes. Herman R. Van Gunsteren explores "Planning as Political Theory," with particular reference to three forms of "new planning": systems-rational, biocybernetic, and communicative. The unique features of the comparative method, as used in political science, are identified by Evert Vedung in a paper on "The Comparative Method and its Neighbors." "On the Use of the Word 'Political,' " by Jan-Erik Lane, is a semantic analysis of the meaning of the word "politics."

The final paper, "Some Conceptual Problems in Political Theory" by Jon Elster, touches on the main themes of all the other essays but is best described as an overview rather than a critique of their theses.

Despite the diversity of topics discussed in these essays, they are linked by a common focus on political theory as an effort to explain the exercise of power within an economic context. Their diversity ensures, however, that all readers will undoubtedly find at least some of the essays of particular relevance to their concerns. At the risk of displaying my personal bias as a political sociologist, the papers on exchange theory by Lively and Birnbaum offer the most stimulating analyses of the role of power in social behavior. The economically oriented reader, in contrast, will probably be more excited by Barry's treatment of power within an economic framework and Lehner and Schütte's sketch of an economic theory of politics. Also worthy of special note, particularly if one's perspective is more philosophical in nature, are the discussions of the relationship between power and causation by Chazel and Oppenheim.

Taken as a whole, this set of essays is on the cutting edge of innovative theorizing concerning the nature of power and politics, and should prove challenging to all political scientists. In addition, by making available to American readers some of the best contemporary European political thinking, the book performs an invaluable communicative service for the discipline. It is not a work to be skimmed lightly, for all of the papers require careful study to be fully comprehended and for their uniformly thoughtful but often demanding arguments to be appreciated. If one is willing to invest the requisite time and intellectual energy, however–and if one is not bothered by the small print size–mastering these essays will undoubtedly be a rewarding experience.

MARVIN E. OLSEN

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Theories of Revolution: An Introduction. By A. S. Cohan. (New York: Halsted, Wiley, 1975. Pp. 228. $7.95, paper.)

In concluding *Theories of Revolution*, A. S. Cohan argues that "unfortunately, the main feature that may be noted about much of the theoretical literature is its thinness and triviality" (p. 211). Unfortunately, the book does not take us beyond that unhappy state, as Cohan chooses to remain largely *au-dessus de la mêlée*, but does serve an important function. As a basic introduction to some major theorists of revolution, the work is concise and serviceable. The author achieves his objective–the reduction of complex, elaborate theoretical positions to simplified, but usually not simplistic, forms while pointing out deficiencies; nothing more is attempted.

Cohan begins with a discussion of "dimensions of revolutionary change," noting that there is a great deal of conceptual confusion and variation from one theorist of revolution to the next. He distinguishes among six distinct meanings of revolutionary change, ranging from the alteration of the values or myths of a society to the violent overthrow of a specific regime. The author recognizes, but unfortunately does not resolve, the dilemma of how to integrate and compare theorists who have widely differing concerns and levels of analysis; the problem of definition and focus is left hanging precariously. There are no consistent distinctions made among theories of forceful regime alteration, theories of collective violence, and theories of social change; "revolution" is used throughout to refer to all of the above and more. Cohan is certainly justified in stressing that regime alteration is different from structural change, and no guarantee of it, but his exosition is seriously weakened by his failure to differentiate among the types of political phenomena which are being explained by his selected theorists.

The absence of analytical consistency is surprising, since chapter 3 explicitly argues the importance in social science of models and formal theory, explanation and causation, concluding with a fine illustrative treatment of Aristotle's theory of revolution. The clarity and rigor of the brief treatment of Aristotle should have provided a model for analyzing the other theorists.

Chapter 4 treats Marx (whom Cohan does *not* charge with "thinness and triviality") and is well done, particularly in contrasting the Marxian model with consensus-oriented models of society. Cohan distinguishes between the basic Marxist model and specific predictive propositions derived from that model, arguing that the supposed inaccuracy of some of those propositions does not invalidate the model, but calls for theoretical refinement. The point could have been strengthened by emphasizing Lenin's discovering in Marx's correspondence explicit reference to the revolutionary potential of peasants, a potential dramatically evident in the twentieth century. The following chapter is a clear and useful discussion of Marxist theories of revolution, concentrating on Lenin and Mao, but including insights from Fanon, Debray, and Marcuse.

Chapter 6 examines the functionalist approach to revolution, emphasizing the work of Chalmers Johnson. Cohan points to the severe problems of tautological explanation and operationalization of concepts in functionalist theory, and also addresses the "problem of bias"–the normative assumptions of the paradigm. The following chapter explores "the theory of mass society" and is much weaker than the preceding chapters. Long digressions on "pluralist democracy" and "marginality" seem slightly off target and the analysis wanders. Important distinctions between the perspectives of Hannah Arendt and William Kornhauser are not adequately discussed. Cohan does, however, examine some crucial conceptual and empirical problems in the primary example of mass society theorists—Nazi Germany. Chapter 8, on the psychological approaches to revolution, is the weakest. The chapter would benefit from conceptual refinements such as those of Fred Greenstein's *Personality and Politics* and a more judicious choice of theorists. Relative deprivation models are subjected to solid but familiar criticisms. There is no attempt to integrate psychological models with theories of structural change; given the author's special concern with the Marxian model, the insights of Jürgen Habermas or Sartre's *Search for a Method* might have provided interesting linkages.

The conclusion reiterates the general conceptual confusion and the absence of bridges between micro-level psychological theories and macro-level structural analysis. Cohan concludes that the Marxian model is the "most satisfactory" but lacks adequate specification of the dynamics of the growth of alienation and class consciousness. My conclusion is that Cohan has done a fine job, with some exceptions, and addresses critical issues that are sometimes ignored. However, virtually no attention was paid to the extensive, rich and sophisticated literature on peasant revolts and revolutions. Nor was sufficient attention given the structural conditions which facilitate or discourage collective political action by those who are embittered and disaffected but are also rational human beings—e.g., the regime's repressive capabilities, or what Ted Gurr calls "the coercive balance," or simply the likelihood of success or survival.

RONALD J. HERRING

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The Practice of Rights. By Richard E. Flathman. (New York: Cambridge University Press, 1976. Pp. vi + 250. $15.95.)

This is a superb book. It does many things, and it does each one well. It is partly an analytical study of the concept of rights, a concept which is neither so obscure that one can hardly bear it nor so superficial that it is boring. Flathman seems to be genuinely gifted when it comes to analysis of the concept of rights and he is able to distinguish usages which are plausible. He is even better at the formidable task of comparing rights with other key concepts. His distinction between rights and authority is especially effective and satisfying. Altogether the strictly analytic side of his work is rich.

Flathman views rights as making sense only as part of social life, as part of "rule-governed social practice" (p. 6) which "cannot be understood apart from" the social environment (p. 69). It follows for Flathman, of course, that "rights are not natural, divine, primitive, or brute facts" (p. 2).

This claim is obviously no modest assertion and it is made perhaps too quickly by Flathman, who does not seem to appreciate the rather dramatic, or even immodest, contention involved in brushing aside fundamental perspectives held by many political theorists, including a good many in our time.

Flathman here takes for granted what he needs to argue at greater length. Moreover, he is so committed to practice as his standard for rights that he really does not want to affirm the existence of universal rights (p. 104), which forces him into a conflict with one feature of what is a widely shared understanding of rights, their universal character. Similarly, Flathman does not have much of a solution to the fact that much discourse about rights speaks of rights which do not exist according to practice. Flathman recognizes that there may be strategic utility to employing what he calls "rhetoric use of rights," but he does not come to terms with the fact that most of the so-called rhetorical uses of rights are employed not as rhetoric but rather because such rights are believed to exist by nature. Finally, his brief treatment of private property rights, relegated to an appendix, will offend some. He does not show much interest in or enthusiasm for the practice of private property rights. His ambivalent and vague treatment of the right to private property suggests that Flathman wishes he didn't have to acknowledge this widespread, important product of practice, and he is not particularly willing to do so. Clearly he likes some parts of practice of rights a good deal more than others.

One of the most interesting parts of this able book is Flathman's unapologetic attempt to defend what he calls "the liberal principle," the prima facie "good" that people should "be in a position to act upon and satisfy their interests and desires, objectives and purposes" (pp. 7, 174). His defense of liberal individualism derives partly from what he considers to be the beneficial consequences of this model of behavior and partly from what he describes as a conceptual fact: "It is a conceptual fact that having and acting upon interests, purposes, and so on, is deeply embedded in our notion of a human being, and it is a fact that this is a normative notion" (p. 169).

To be sure, Flathman grants that the liberal principle is an prima facie principle only, so that he willingly accepts restrictions on individual freedom as long as reasons are given which take into account social and individual consequences. He wants nothing to do, though, with those who delight in being "moral meddlers" (pp. 177–78).

Finally, Flathman sets out to defend rights against the usual charge that they are irreconcilable with community, and vice versa. He attacks natural-rights thinkers on just this ground. For him they are committed to an atomistic view of human behavior which has led to the belief that rights and community are two ideas in conflict. This belief is grossly "exaggerated" and commonly "misrepresented," Flathman contends, as long as we do not enter onto the false natural-rights path (p. 196). If we follow him, Flathman says, and observe that rights are practice, then we know that the conflict between rights and community is not so obvious. After all, Flathman suggests, in practice rights are social in that they proceed under a common language and common rules for their operation which negate any atomistic overtones. Flathman looks to a liberal polity where a "private" form of citizenship will not dominate a more "civic" attitude, where rights operate in a framework of concern for the common good (Chs. 9 and 10).

ROBERT BOOTH FOWLER

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Controversies and Decisions: The Social Sciences and Public Policy. Edited by Charles Frankel. (New York: Russell Sage, Basic Books, 1976. Pp. x + 299. $10.00.)

In the western democracies, the classic response to social unrest or upheaval has been the formation of a committee of notables which in due course and after suitable inquiry delivers a report—the length, complexity, and utility of such reports vary immensely. In the academic community, the principal device for relieving syndrome pressure has been the symposium. Ordinarily, symposia develop in a fairly systematic way: an entrepreneurially oriented professor locates, either by accident or by design, an issue, crisis, or "problem" which is deemed marketable and generates a symposium to fit. If the design can be sold to a sponsor, the entrepreneur invites contributions, arranges meetings, coordinates production, and in time edits and publishes the finished product. Considering the work involved, the number of professors willing to undertake the task for little more than honorific recompense is astonishing.

A properly organized symposium brings a number of different but relevant conceptual structures to bear on some common problem; each can reveal aspects of the situation that are opaque to the others. The result is superior illumination and greater potential for solution of the problem involved. The procedure makes possible, if it cannot guarantee, development of mutually reinforcing interpretations of events and exploration of various alternative routes to problem solution that can be combined in different ways at different costs. However, the very hydra-headedness which gives the symposium its potential value is a prime source of weakness. The logistical problems alone are formidable, for they seem to increase exponentially with the number of authors involved. Time delays required to schedule meetings and coordinate production can "date" the finished product before publication, particularly for symposia organized around current issues. The intellectual perils inherent in the form are more serious. The fact of a common target need not lead to mutually supportive judgments or interpretations; too often, the levels of sophistication incorporated into the various essays differ so greatly that the results do not intersect at all, producing a symposium that integrates its essays in much the same way that a fruit basket "integrates" different kinds of fruit. Again, the search for common ground can lead to banality rather than generality, resulting in apologia rather than analysis. In sum, while the form can be useful for illuminating various facets of social and other problems, it is not a particularly useful tool for problem solving.

Charles Frankel's symposium, a response to the university student outbursts of the 1960s, avoids most of the major pitfalls. The editing is excellent and strongly reinforced by an introductory essay in which the topics discussed are blended very skillfully into the narrative. The individual essays are for the most part well written and on target. The only essay to which I reacted quite negatively, perhaps because of a personal aberration, is a lengthy piece on the reward system in the social sciences by Jonothan and Stephen Cole. It struck me as both conceptually disparate from and qualitatively inferior to the rest of the contributions—because of their choice of indicators, the authors at times come very close to arguing that lawyers who win most of their cases must defend more innocent people, for example.

The overall volume is a thorough and competent survey of some of the fundamental issues raised by the uproar in the universities, placed in a brief but adequate historical context and illustrated in quite a useful way from current history. Generally, I found the "philosophic" essays both more interesting and more compelling than the case studies. Six of the fourteen essays are focused on the central conceptual issues involved in the controversy–autonomy of social science, objectivity, value neutrality, and the fact-value dichotomy. Their individual merits are substantial, and one, Kenneth Boulding's "Scholarly Rights and Political Morality" is perhaps the best short summary of the relation between government and scholarly community I have seen anywhere. The compendium also includes essays on the Jensen controversy specifically, on mental testing more generally, and on the relationship between the federal government and higher education and the problems involved for both sides. A short but informative piece by Adam Yarmolinsky illustrating the policy maker's perspective on government-academic relations adds an unexpected and most welcome bit of seasoning to the mixture. Students or faculty not familiar with the issues raised in the 1960s over the nature, function, and purposes of universities in general and the social sciences in particular will find the fundamentals well summarized here.

The volume is not faultless. Despite the often articulated concern for the introduction of unintended biases, the symposium is dominated by a point of view which is probably most representative of senior faculty in the more prestigious private universities. Both the editor and his contributors make claims on behalf of the fields of economics that seem grossly exaggerated in the light of governmental foundering in the 1970s, despite the best advice economists could offer (p. 184, for example). And if the symposium deals effectively with the conventional dimensions of the issues, it does not go very far beyond them. Those already familiar with these discussions will find little to command their attention or assist their thinking here.

My principle demur, however, concerns the underlying conception of the nature of the scientific enterprise that permeates the work of both editor and essayists (especially pp. 26, 267, 290). It implies and requires a separation of the scholarly role from the affairs of society that would be disastrous if maintained. When Frankel asserts (p. 26), "The possession of a powerful analytic structure means that the science has within it a factor making for its development along lines independent of existing practical demands or moral beliefs," he far exceeds the claim implicit in the scientists' demand to pursue "theoretically interesting" questions. What is omitted is the distinction between *development* in science and filling holes in the theoretical latticework of a field. While the two activities may overlap, they need not. And accepting the position that Frankel states here tends to the kind of mathematical formalism that substitutes the computer for the laboratory, the postulate for the observation, the evolution of logics for their application (not itself a problem in logic). The end result is yet another metaphysics and not science. To that I object strenuously.

EUGENE J. MEEHAN

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The Collapse of Liberal Empire: Science and Revolution in the Twentieth Century. By Paul N. Goldstene. (New Haven and London: Yale University Press, 1977. Pp. 139. $10.00.)

The liberal mentality, according to Goldstene, thoroughly permeates American beliefs and attitudes. Even its detractors affirm it unwittingly. Nonetheless, liberalism is in crisis because, first, the institutional means to its economic objectives of growth and affluence defeat its political commitments to individual rights and pluralist balance; second, the very production of selective affluence undermines the allegiance of youthful beneficiaries to the roles available to them in the corporate system; and, third, the growing resistance of third-world countries to the expansion of American corporations causes the progressive internalization a variety of pressures and sacrifices which previously had been exported.

Newtonian theory provides the root metaphor through which the liberal mind comprehends and evaluates the political order. If "all things in motion are in opposition," then politics must embody a "Newtonian conception of balance where the proper arrangement of action and reaction eventuate in symmetrical arrangement and proportion." But it is exactly this imagery–the imagery of balance, pendulum shifts, cycles, trade-offs, checks, and backlash–that blinds liberal ideology to the emergence of a corporate state and limits its ability to respond to these new contradictions. Indeed the language of balance is incompatible with that of contradiction; the two vocabularies belong to qhite distinct and opposed traditions of understanding. An outlook oriented to correcting imbalances will not seek ways to "transcend" or "resolve" social contradictions.

But what does Goldstene affirm? He looks to the scientific estate to establish, well, a new balancing force appropriate to the new age. He anticipates a new liberalism in which science, "vague as its present power may be," delivers the "organizational promise of balance." This new balance is not to operate (I think) primarily between institutions, but is to be insinuated within institutions and individuals. A "new quality of mind" will balance the present emphasis on instrumental modes with a contemplative, appreciative orientation to social relations and nature.

Goldstene's polemic is vividly articulated. But, since it bears a family resemblance to other critiques of liberalism, we might reasonably expect him to refine or perfect one of its dimensions. He might (1) document the extent to which this liberal mentality permeates the public mind, (2) examine closely the ways in which the metaphor of balance constrains the thought of one or two pivotal liberal thinkers, (3) delineate in detail the contradictions in the prevailing order (and exactly what it means to have "social contradictions"), or (4) exemplify the political promise of the scientific estate. Goldstene is most interesting in his discussion of the liberal metaphor of balance. But, with Locke, Madison and Galbraith floating in and out of the text, neither the metaphor nor its intellectual bearers receive the concentrated attention required. In all four respects the argument is suggestive in conception, yet programmatic and abstract in delivery. Indeed, argument (4) remains unacceptably obscure.

This is a provocative, if unbalanced, book. We can expect the author, who believes the "drive for harmony" informs "all action and thought," to restore balance in his later work.

WILLIAM E. CONNOLLY

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Problems of Theory in Policy Analysis. Edited by Phillip M. Gregg. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. 208. $15.00.)

This book of essays draws upon theoretical issues and conceptual problems raised in the course of policy research at Indiana University's Workshop in Political Theory and Policy Analysis. According to the editor, the essays were selected and arranged to highlight four problem areas, summarized by the following four questions, which together form the four major sections of the book (p. 1): (1) Given the nature and objectives of policy analysis, what is a viable role for political inquiry? (2) What models are used to analyze and evaluate policy and how can they be assessed? (3) What problems exist in the conceptual premises that are used to organize policy studies, and what new directions are developing? (4) Given the nature and complexity of policy arrangements, what alternative research techniques facilitate systematic study?

There is little that is new or original about the theoretical issues and conceptual problems discussed in his collection of essays. The volume promises more than it delivers—a characteristic all too often exhibited by much so-called "theoretical" policy studies literature of the past few years. For example, from reading these essays, we learn that politics is a complex process; that politics can have an impact on policy outcomes; that political systems are best conceived as open ones embedded in a complex set of interdependent relationships with "outside" political actors; that much policy research has been deterministic and mechanistic, utilizing simplistic models that distort a complex reality and do not deal adequately with ongoing change processes; that centralized versus decentralized political structural and authority arrangements do not necessarily lead to more efficient, effective, or responsive governance; that conceptual clarity is important, and so on. All of which has been said before, and I am sure will be reiterated again, and again.

However, these essays do point to the pressing need for new theoretical frameworks to guide empirical policy studies research. The most potentially useful contribution to this need is Phillip Gregg's article entitled "Toward Theoretical Premises for Policy Analysis," which contains some interesting and empirically "testable" theoretical ideas.

Gregg draws upon political scientists' historical tendency to study "the structure of political institutions and their consequences for the processes of government, the public order, and the commonwealth" (pp. 151–52). He argues that three conclusions can be derived from a view of "policy studies" as focusing on authoritative rules: "(1) rule systems potentially exhibit great variability; (2) variants in rules are primary determinants of differences in the choice and behavior of human individuals; (3) the existing variabilities highlight the artifactual nature of rules; they are primary subjects of human evaluation and choice in the transformation of societies—from the level of neighborhoods to nations" (p. 152).

From this follows Gregg's "intellectual challenge" to policy studies: "we must amplify our capacity to study the variations among rule structures and their consequences for human welfare. The analytic concepts must: isolate the primary constituents of policy systems, distinguish among types and patterns of rules that constrain decision making, and facilitate comparative study of the human consequences of alternative rule configurations" (p. 152). Gregg then sets forth primary definitions of terms, premises and their implications, selected inferences, and finally, a framework for study incorporating this perspective. As in many areas of political science these days, it appears that

Gregg has brought us full circle, with some obvious contemporary nuances, back to the systematic study of rules and their consequences for human welfare. Back to our roots?

Gregg's discussion of applied problem solving research is also more potentially useful than that of other authors in the collection. Instead of a "'disjunction' between 'applied' policy analysis (i.e., research to improve policies or solve problems) and 'pure' policy research (i.e., that conducted to test hypotheses or establish generalizations)" (p. 171), Gregg argues that the basic concepts and inferential premises he presents can be used to organize and show the mutually reinforcing relationship between both types of research.

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John Stuart Mill. By R. J. Halliday. (New York: Barnes and Noble, 1977. Pp. 151. $12.50.)

The greatest obstacle to a satisfactory understanding of John Stuart Mill is his vast array of seemingly incompatible opinions on the most important questions he addressed. He endorsed democracy and yet proposed many obstacles to rule by the people; he defended laissez-faire, yet also displayed a good deal of sympathy for the socialist ideal; he wished to maximize liberty, but he has been called the best critic of the views found in the essay *On Liberty;* he insisted that he was a utilitarian, yet he was a severe critic of Bentham and a defender of Coleridge.

These varied opinions and arguments have led some to the conclusion that Mill was self-contradictory; others, however, have tried to show that Mill's views were expressed with sufficient care and qualification to make them logically compatible. R. J. Halliday aligns himself with the latter group, his major theme being that Mill was an eclectic and that his eclecticism allowed him to accommodate a considerable diversity of views. Halliday suggests that one finds self-contradictions in Mill if one attaches to Mill labels with overly simple definitions that Mill himself would not have used. Thus Mill's wish to provide for intellectual leadership may be regarded as incompatible with democracy only if we attribute to him a belief in populist democracy which he did not hold. Or his ideas about competition and socialism may be regarded as contradictory only if we assume that Mill defended laissez-faire as we characterize it.

Halliday's thesis about Mill's eclecticism, it should be said, applies only to Mill after he recovered from his mental crisis, i.e., to the period beginning around 1830. Even the author's comprehensive and subtle analyses cannot show that the ideas of the Mill who wrote the famous books and articles are compatible with Mill's orthodox Benthamism before the mental crisis. Thus Halliday begins his book by briefly describing the mental crisis as the occasion for the intellectual turbulence that led to the mature thinker.

There is no novelty in calling Mill an eclectic. Indeed, Mill made it clear that he would deliberately seek eclecticism, a decision following his disillusionment with and reaction against "the philosophy of the eighteenth century" as exemplified by Bentham. At this time he advocated a combination of ideas ("half-truths," he called them) taken from such varied sources as Bentham and Coleridge, de Tocqueville and the St. Simonians, romantic poetry and associationist psychology. In view of Mill's widely acknowledged eclecticism, Halliday's contribution consists of detailed demonstration of the ways Mill combined the varied ingredients of his thought. By examining the great variety of statements made in Mill's books, articles and letters, Halliday shows how many of Mill's assertions, even when they appear to be incompatible with one another, may be fashioned into a coherent outlook. He does this for Mill's views on politics, economics, psychology, and ethics, and he also shows how Mill's ideas in each of these areas became intertwined with each other. For example, he demonstrates that Mill's belief in the importance of self-culture and self-development, which originated in his romantic notions that accompanied his reaction against his father's educational beliefs and practices, contributed to the shaping of his belief in laissez-faire as encouraging the exercise of each person's mental faculties. The same belief in self-culture is also shown to have entered into Mill's understanding of democracy giving opportunities for participation in politics.

Although Halliday convincingly displays the features of Mill's eclecticism in some areas, his theme is not without limitations, which he acknowledges in the preface. After all, eclecticism was Mill's goal, but there are many indications that he failed to satisfactorily combine the diverse ingredients in his thought into a coherent philosophy. There are other difficulties which may be mentioned. Halliday overemphasizes Mill's pragmatism, and does not acknowledge the sectarian and doctrinaire dimension of his personality and his thought, even after his reaction against Benthamism. In addition, the book offers much understanding but nothing resembling critical evaluation. Furthermore, one could argue that he does not adequately acknowledge the importance of the disinterested intellectual in Mill's political philosophy. Despite such problems, the book has the merit of avoiding hackneyed and misleading labels; and when the author uses labels of his own, he usually qualifies them sufficiently so that it is difficult to ignore their limitations. Consequently we are made aware of the complex texture of Mill's thought.

JOSEPH HAMBURGER

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Frantz Fanon: Social and Political Thought. By Emmanuel Hansen. (Columbus: Ohio State University Press, 1977. Pp. xi + 232. $15.00.)

Although the enthusiasm and outrage initially aroused by his work has largely subsided, Frantz Fanon remains a revolutionary figure of imposing authority: where others of the era (such as Regis Debray) merely issued a call to arms, Fanon became a poet of emancipation; where others merely denounced imperialist domination, Fanon became the psychoanalyst of cultural degradation. Notorious in the sixties for its alleged apotheosis of violence, Fanon's lifework endures in the seventies as a paradigm of intellectual commitment, illustrative of the problems as well as the possibilities any such commitment creates. By 1961, Fanon had become deeply involved in the Algerian war for national liberation, and, more broadly, the international struggle for dignity within the Third World. He remains one of that struggle's most eloquent voices, still able to touch our emotions and engage our minds, despite all the years that have come between.

Emmanuel Hansen's book promises a "clear understanding of Fanon's thought, based on a study of the totality of his ideas, using the entire corpus of his writings" (p. 10). It is an admirable intention. For Fanon has always seemed an enigmatic thinker, driven by a unique complex of forces and ideas. Superficially, he owes most to the great traditions of *négritude* and Marxism; yet he was also influenced by Jacques Lacan, the currently fashionable French psychoanalyst, and Jean-Paul Sartre, whose philosophy and politics provided a model of independence. The biographical question seems absorbing: how does a sophisticated black psychoanalyst, born in Martinique, educated in France, and a practicing analyst in Algeria, come to portray the modern North African peasant as a kind of "noble savage" in revolutionary garb?

Emmanuel Hansen, however, eschews questions of biography, influence and conceptual articulation; nor does he bother much with comparative analysis. For example, no attempt is made to place Fanon's theory of revolutionary violence within the modern context supplied by the moral exaltation of George Sorel and Sartre, the tactical justification of Lenin and Trotsky, or the historicist apologetic of Georg Lukacs and Maurice Merleau-Ponty.

Instead, Hansen devotes himself to exegesis. Unfortunately, it is not very good exegesis. The author treats Fanon's work as an undifferentiated corpus; typically, he writes (on p. 39) as if there were no important difference between the neo-Hegelian notion of a "struggle" for recognition in *Black Skin, White Masks* (published in 1951), and the neo-Sorelian notion of "revolutionary violence" in *The Wretched of the Earth* (published in 1961, the year of Fanon's death).

The book's most serious weakness, however, is the systematic way in which Hansen takes a writer consumed by an almost palpable passion, and reduces him to the dimensions of an anemic and rather dull theorist. To be sure, we are reminded that "Fanon wanted to be at the center of things—where the action was. We must not forget his commitment to praxis" (p. 38). But such lifeless clichés fatally resolve the tension which animates Fanon's lifework.

The Wretched of the Earth, for example, is shaped by the divergent demands imposed by Fanon's ruthless assessment of violence as a tactic in wars of national liberation; by his emotional and highly rhetorical defense of freedom, at whatever the cost ("hunger with dignity is preferable to bread eaten in slavery"); and by his sobering analysis of the psychoses triggered in some of his patients by the experience of torture and terrorism. Like previous critics, Hansen is alert to the apparent contradiction between the book's first chapter, "Concerning Violence," and its final chapter, a series of clinical studies on "Colonial Wars and Mental Disorders." Yet while he briefly puzzles over this indicative tension (p. 127), Hansen does not make any sustained attempt to explore it. As a result, his interpretation minimizes the ambiguity and pathos of Fanon's position. Rereading the last chapter of *The Wretched of the Earth,* I was reminded of a line from *Man's Fate*, Malraux's great revolutionary novel: "What is deepest in man is rarely what one can use directly to make him act." That, I suspect, is one bitter truth that Fanon, as a psychoanalyst as well as a revolutionary, knew all too well.

JAMES MILLER

University of Texas, Austin

Law, Legislation and Liberty, Volume 2: The Mirage of Social Justice. By F. A. Hayek. (Chicago: University of Chicago Press, 1977. Pp. xiv + 195. $10.00.)

Reviewing this book is a bit like reviewing the second act of a yet incomplete three-act play. Hayek is engaged in a three-volume work under the general title *Law, Legislation and Liberty.* The first, *Rules and Order,* was published in 1973, followed four years later by the volume under review. The final volume apparently is already in draft, and it will be titled *The Political Order of a Free Society.* He has indicated that he would have titled his project *The Constitution of Liberty* had he not given that name to his earlier work.

It is no insult to say of Hayek that he has one of the finest minds of the eighteenth century. He specifically identifies himself with the tradition of thought linked with David Hume, Adam Ferguson, and Adam Smith, one which he sees as having laid the basis for the creation of the "Great Society," that is, a social order featuring consensus only on procedural rules of personal interaction. These procedures are allegedly end-independent, and individuals within this society are free to pursue whatever aims they wish, limited only by the duty to remain within the boundaries established by the rules. Hayek is less an analytical philosopher than an amazingly erudite guide to what he views as the already existing widsom to be found in the thought of the men mentioned above.

Rules and Order is devoted largely to an elaboration of "law" as grounded in evolutionarily given (and naturally just) social practices, rather than as the product of some sovereign "will," whether of an individual ruler or the democratic populace. *The Mirage of Social Justice* attacks the notion that one can evaluate a social order on the basis of any one standard of social justice. A free society, according to Hayek, is one allowing the pursuit of multiple ends, and to adopt a single standard of evaluation is thus to deny the reality of pluralism. Indeed, he even rejects the existence of a national "economy," on the ground that only individuals can have "economies," that is, allocate their own resources in accordance with a rank-ordering of ends. A society therefore is a collection of individual economies. He coins the word "catallaxy," "the special kind of spontaneous order produced by the market through people acting within the rules of the law of property, tort and contract" (p. 109), as a substitute for the term "national economy."

No devotee of free-market ideology will be converted by any criticisms I might make of the problem involved in viewing contemporary society as the product of "spontaneous order," nor will any of his opponents be turned into admirers by reference to his strengths. Hayek presents elegantly, and lucidly, a view which has undergone a remarkable revival in the past decade, not least because of a widespread disillusionment with the activities of the modern nation-state. And, although he presents a rigorous defense of the necessity to accept the often unattractive results of the operation of the catallactic order he describes, on the ground that any "cure" would be worse than the disease, Hayek is sensitive to some of the criticisms leveled at an unrelieved market.

He thus admits that "capitalism as it exists today ... undeniably has many remediable defects that an intelligent policy of freedom ought to correct. A system which relies on the spontaneous ordering forces of the market, once it has reached a certain level of wealth, is also by no means incompatible with government providing, outside the market, some security against severe deprivation" (p. 136). This is only one of several passages where Hayek, a *doyen* of classical liberalism, sounds very much like John Rawls; indeed Hayek indicates (p. xiii) that he views himself as in substantial agreement with Rawls' widely read (and praised) *A Theory of Justice,* any differences between them being "more verbal than substantial." There are certainly Rawlsian overtones in Hayek's ultimate standard for the best social order: "that in which we would prefer to place our children if we knew that their position in it would be determined by lot" (p. 132).

One suspects that admirers of Hayek and Rawls will be equally discomfited by the suggested similarity, but one of the major benefits of reading *The Mirage of Social Justice* is the realization that much of the alleged difference between them may turn on empirical questions rather than on truly philosophical ones or differences in normative values. Thus Hayek over and over again insists that a free-market order will in fact lead to the largest social benefit, including as beneficiaries those at the lowest end of the social order. The catallaxy increases "the prospects or chances *of every one* of a greater command over the various goods (i.e., commodities and services) than we are able to secure in any other way" (p. 107, emphasis added). Rawls, who shares Hayek's commitment to the priority of liberty, does not himself condemn inequality *per se,* but only that inequality which does not ultimately benefit those worst-off, through the creation of more overall (the properly distributed) social wealth. Rawls scarcely resolves the question of how much inequality is "necessary," and his own arguments can easily lend themselves to a sophisticated defense of substantial inequality.

Probably the greatest difference between Hayek and Rawls lies in the latter's much greater sensitivity to the problem of generational equity. Hayek is quite unconcerned about the position of children and concomitant problems of genuine "equal opportunity." It is certainly not crystal clear that commitment to freedom entails acceptance of unrestricted rights of inheritance; Hayek does not discuss this point at adequate length, and it would be interesting to know exactly how much of Rawls' program he is prepared to accept.

In any case, the celebrated battle of the books between Rawls and his libertarian colleague Robert Nozick (whose *Anarchy, State and Utopia* seems unknown to Hayek) has blinded some to the extent to which Rawls is still within the classical liberal tradition, including its free-market emphasis, and Hayek's embrace of Rawls as an ally may tell us much about the strengths, and weaknesses, of both men's thought.

SANFORD LEVINSON

Princeton University

The Passions and the Interests: Political Arguments for Capitalism before Its Triumph. By Albert O. Hirschman. (Princeton, N.J.: Princeton University Press, 1977. Pp. x + 153. $10.00, cloth; $2.95, paper.)

Hirschman traces the convoluted development of theories advocating commercial society in seventeenth- and eighteenth-century political thought. His major thesis is that arguments for capitalism originated in purportedly realistic proposals for restraining the passions of the governed—by interesting them in material pursuits—and of the governors, by setting them over an economic system that was the result of the play of those material pursuits and whose workings stymied arbitrary or selfish princely intervention.

In part 1, Hirschman explains that early modern political thinkers hit on the idea of "countervailing passions." Some passions, relabeled "interests," might be opposed to other, undesirable passions that produce tumultuous, destructive, or frivolous actions. While reason, for instance, tends to be inefficacious against the passions, the interests, because they are passions, can tame the passions. In individuals, interests can and should oppose and conquer passions so that the individual acts in a regularized, nondestructive, serious way to define and pursue life goals. The most important taming interests are those related to making money, which *"could be usefully employed to oppose and bridle such other passions as ambition, lust for power, or sexual lust"* (p. 41). Since the individual's pursuit of material interests restrains unruly and destructive passions, commerce is seen as "innocent," "*doux*," and "calm." Money-making is milder in its effects than the aristocratic passions for honor and glory. Further, a society of materially self-interested individuals is predictable and constant in behavior. The spirit of capitalism began as a way of restraining all other human passions.

In part 2, Hirschman explains how the political theorists relate interest to government. He focuses on Montesquieu, Sir James Steuart, and John Millar, who saw that individual pursuit of material self-interest and its attendant economic expansion served to improve the political order by curbing the passionate excesses of rulers. Steuart, for instance, argued that, because a modern economic system has the complex interdependent workings of a watch, a powerful government cannot attempt arbitrary acts, since such acts would disturb the smooth functioning of the system and undermine the governors' intent. But there were discordant views, and Hirschman's history ends with Adam Smith, who rejected the distinction between the passions and the interests (and its attendant arguments) and saw few ways of restraining rulers who would be wayward.

In an epilogue-like and disjointed final part, Hirschman reflects on some problems of the "Montesquieu-Steuart vision." He contrasts this interpretation of the rise of the capitalist spirit with Weber's. And he makes some "contemporary notes" about critiques of capitalism and about intellectual history.

Hirschman's interesting thesis deserves serious examination; unfortunately, Hirschman's essay does not provide that examination. Although his topic places him in the company of Macpherson and Pocock, his essay is thin by comparison, even slight in length, although this is a minor consideration. Hirschman thinks he is presenting novel interpretations, but he does not confront opposing interpretations. At the most general level, he does not argue against the theses of Macpherson or Pocock, Marx or Weber. More specifically, he does not criticize current interpretations of Montesquieu or Adam Smith, for instance; he simply presents his own, but without substantial enough articulation, arguments and documentation to be convincing.

Hirschman's essay also lacks rigor. Countercases are left hanging. While Montesquieu represented a chief example of the theory of commerce as "calm" and desirable, at the same time he feared the "monetization of all human relations" (p. 80); Hirschman leaves the reader unsure as to how Montesquieu resolved this tension. Hirschman's criteria for determining important or pervasive views are unclear; while Millar's ideas are presented as essential to the "Montesquieu-Steuart vision," Hirschman seems to indicate that most of Millar's contemporaries disagreed with him (pp. 88, 92).

Two further criticisms, with many implications, can be suggested here. One is that Hirschman inaccurately sees the same type of developmental unity in explicit theorizing as in unconscious assumptions. In part 1, he validly presents as a unified intellectual development the "tacit dimension" of two centuries of thought about passions and interests. In part 2, where he shows theorists explicitly treating the implications for political economy of that tacit dimension, Hirschman's approach is to show divergent explicit views under the inaccurately unifying guise of a development. Of the eighteenth-century theorists he treats in detail, he presents (on little evidence) "Montesquieu-Steuart" as holding the predominant view, the Physiocrats as a competing school, and Adam Smith as finally victorious, relegating Ferguson to the epilogue because he does not fit Hirschman's developmental sequence, even though Ferguson's major work was published in the same year as Steuart's (1767) and shows a perception of the totalitarian implications in Steuart's thought. While tacit agreement may permeate the thought of an epoch, explicit conclusions may—and here do—vary widely. Hirschman mistakenly presents both dimensions as equally general.

Second, while he generally traces the meaning of concepts well, Hirschman nowhere treats "capitalism." Since according to the *OED* the word was not current until 1850, Hirschman's theorists did not consciously argue for capitalism, but for commercial society. Hirschman reads their thought too exclusively from his vantage point of a member of a realized capitalist society, without considering that he may thereby mistake their emphasis where they see different trajectories or goals for commercial society.

To some extent, my criticisms stem from a sense of potential unrealized. Hirschman's thesis *is* important, and he presents it with twists not found in prior similar arguments (e.g., by Straussians). Everyone interested in modern political theory and in political economy must know and confront Hirschman's thesis. But the thesis needs more work to be convincing.

PETER G. STILLMAN

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Political Violence: A Philosophical Analysis of Terrorism. By Ted Honderich. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. x + 118. $7.95.)

Violence is indubitably an overshadowing problem in our day. One can think of it in sociological or psychological terms; one can chart its many dimensions and seek to explain it, or, as Ted Honderich seeks to do, one can inquire into its justifiability. Honderich's book is termed "a philosophical analysis of violence."

The author is centrally interested in the justifiability of *political* violence, which he defines as "a considerable or destroying use of force against persons or things, a use of force prohibited by law, directed to a change in the policies, personnel or system of government, and hence also directed to changes in the existence of individuals in the society and perhaps other societies" (p. 9). But he narrows his definition considerably when he excludes both "violence at the level of civil war" and "war between nations" (p. 10). He has in mind "the level of violence with which we have become familiar in Britain, America and elsewhere in the world during the past decade" (pp. 9–10).

He begins by outlining in sympathetic terms the modern problem of inequality, which, he thinks, violence is often used to correct. He then proceeds to examine certain general doctrines for the light they might shed on the morality of political acts. In the central part of the volume, he comments on types of reasoning bearing on our obligation to obey the law, examining the utilitarian principle, John Rawls' theory of justice, and his own criticism of Rawls' notions. The last portion of the book is devoted to his justifications for certain types of what he calls "democratic violence."

Because the analysis is so condensed and lacking in concrete illustrations, the reader may at many points find it difficult to follow. The end result, however, is clear enough. Rejecting the contentions both of those who oppose all political violence on principle and of those (and here he mentions Sartre and Fanon) having a strong and passionate commitment to violence, Honderich concludes that "at least some violence has a moral justification" (p. 116). Then he immediately and curiously adds: "But I have not done anything like show this" (p. 116).

In general, he seems to be saying that in limited degree (how limited we are not told) and at a level considerably below that of civil war, violence may under certain circumstances be vindicated in the pursuit of social and economic equality. But in developing this analysis, he appears to make many statements which are ambiguous and rather puzzling. For example, "We may indeed assert that the state may force a man to act, as we say, against his conscience" (p. 59). At no point, however, is "conscience" defined and no example is provided to show how the state can "force" a man to act against his conscience. He criticizes "claims to the effect that acts of violence themselves *destroy* democracy." In general, he thinks, "they do not. They do less to make systems undemocratic than do acts of economic power" (p. 102). This may well be true but one wishes he had gone a bit beyond the bald statement of the proposition.

When he says, "The proposition that violence does as a matter of fact promote progress toward freedom and equality in some circumstances can hardly be questioned" (p. 110), one wonders whether he ever heard of a serious questioner like Gandhi. "The nostrum that nothing is gained by violence does not survive a moment's reflection," he goes on (p. 110). But what does the author understand this "nostrum" to mean? If it signifies that political violence cannot accomplish anything, desirable or undesirable, then obviously it is incorrect; for at a minimum, violence does effectively destroy things and human lives. If one interprets the expression to mean that "nothing just or democratic" is gained by violence, then surely such a statement is more than a mere "nostrum." Would Honderich dismiss the faith and experience of the Quakers as having no bearing on the issue? Or the generalizations, surely based on more than a mindless repetition of slogans, of men like Leo Tolstoy? It is surprising, too, that Honderich does not even refer to the notion, so prominent in writers like John Dewey and Aldoux Huxley, that means and ends are organically interrelated. Nor does the author mention the possibilities of nonviolent resistance, which surely ought to be regarded as an alternative to violence, on the one hand, and acquiescence in gross inequality, on the other.

In many respects, this is a thoughtful essay. But one wishes that Honderich had been more concrete and had provided more illustrations to illuminate his abstractions.

MULFORD Q. SIBLEY

University of Minnesota

Justice, Human Nature, and Political Obligation. By Morton A. Kaplan. (New York: Free Press, 1976. Pp. xix + 283. $12.95.)

Alienation and Identification. By Morton A. Kaplan. (New York: Free Press, 1976. Pp. xiii + 206. $10.95.)

Morton Kaplan has written in earlier books on systems theory and the methods of social science, and in the volumes under review he has attempted to apply ideas developed earlier to some central questions of political philosophy. The two books are clearly part of a single project, and their separate existence remains unexplained. Kaplan has to repeat arguments, which is irksome to someone reading both volumes.

The common basis is a view of human beings and societies as homeostatic systems, striving to maintain equilibrium with changing environments. This implies, Kaplan argues, that general theories cannot be produced in the social sciences, and that our explanations of individual and social behavior must be achieved through a synthesis of partial theories, requiring a process of judgment that Kaplan refers to as "praxis" (which is odd, since no reference to practical activity in the usual sense is intended).

Thus armed, Kaplan turns, in *Justice, Human Nature and Political Obligation*, to an analysis of goodness and justice. His first concern is to argue that both notions are relative to particular social systems, so that "good" refers to "those things that improve the state of the (social or personality) system," and this forms the basis for a long attack on Rawls' theory of justice, which is criticized for attempting to establish universally valid principles. This line of argument leads, however, to moral relativism, by which it is possible to assess actions or rules in relation to particular societies, but not to compare societies themselves. To avoid this conclusion Kaplan undertakes to demonstrate the objectivity of values, aiming to reach a higher-level criterion of goodness that can be used to make relative assessments of whole societies.

He prepares the way by arguing that supposedly objective empirical judgments—like "this is yellow"—tacitly presuppose an observer and a context of observation. If the observer's sense organs, or the physical environment, were to alter radically, the judgment would no longer hold. Kaplan claims that judgments of value are similarly objective in principle, but here contextual factors are more intrusive and cause practical disagreement. There is, however, a "test in principle" whereby the goodness of social systems may be assessed. We are invited to examine different systems by thinking ourselves into the various roles that they contain, and seeing how much we like them (presumably we are to aggregate our feelings for each system to arrive at an overall ordering).

This position is open to several damaging objections. First, the analogy between empirical and moral judgments does not hold. While it is true that if (say) our organs of sense were constructed differently, we should make different perceptual judgments, it is precisely because we do in fact agree (almost unanimously) in these judgments that we can speak of them as objective. With moral judgments, however, there is not merely *hypothetical* disagreement but *actual* disagreement, and thus the problem about objectivity arises. Second, the "test in principle" presupposes that moral disagreements arise only from imperfect information and/or personal bias on the part of the people making the judgments. It seems likely that there are value differences that cannot be accounted for in this way. Suppose I am using Kaplan's test to assess a hunting-and-gathering society, should I judge each role according to how much *I* should like to occupy it, or should I try (supposing this were possible) to imagine how a member of *that* society would feel? Third, Kaplan's test, because it aggregates likes and dislikes in arriving at social evaluations, is necessarily insensitive to questions of distributive justice—questions about the relative levels of benefit enjoyed by the occupants of different social roles. As a result of applying it we might come to prefer a highly unequal society whose inhabitants were unaware of the inequality.

I have criticized Kaplan's argument for the objectivity of goodness because it seems to me the key element in his first book. By contrast, the chapter on justice is largely devoted to attacking Rawls and, although the criticisms are quite incisive, they are by now not particularly novel. Kaplan has little to say about justice itself, and indeed gives no clear criterion for distinguishing justice from goodness in general. The final chapter on political obligation is likewise flawed by being insufficiently specific. Kaplan offers some wide-ranging observations on the nature of obligation and the dilemmas that may arise when obligations conflict, but does not produce any detailed arguments about the grounds and limits of our obligation to the state.

Alienation and Identification may be dealt with more briefly, since it is a slighter book, and might with profit have been incorporated into the other in condensed form. Kaplan begins by discussing Hegel and Marx on alienation, and then Althusser and Habermas as interpreters of Marx. This leads to a lengthy exposition of Kaplan's views on knowledge and explanation in the social sciences. Part 2 is an examination of the problem of alienation, conceived very broadly to mean the separation of individuals from the products they have created, from their society or culture, or from parts of their own personalities. Kaplan's position can be stated briefly: alienation, far from being uniformly evil, is necessary both to society (which requires, for instance, a division of labor to fulfill essential functions) and to individuals who need, for instance, to distance themselves from their creations in order to progress). Theories which announce the end of alienation are misguided. Furthermore, practical attempts to end alienation, whether carried out on the political level (through revolution) or on the personal level (through adopting an idiosyncratic lifestyle) produce derangement and ultimately, perhaps, madness. The conclusion is that we must accept a degree of personal alienation, and learn to cope with the multiple identities that modern societies provide for us.

The trouble here is that by construing alienation so broadly and imprecisely, Kaplan has made his conclusion more or less self-evident. Who has imagined that alienation in every conceivable sense could be eliminated? Marx, for instance, was chiefly concerned with the alienation of workers in the process of production, and Kaplan does not really meet the claim that alienation of this type might be eliminated or reduced if work were organized differently. But it is surely at this relatively specific level that interesting arguments about alienation can be carried on.

The basic failing of both books should now be clear. Kaplan's argument is developed at too general a level to constitute a satisfactory treatment of the particular topics he includes. His breadth of knowledge is impressive. You will find here discussions of the physiology of the human brain, the external policies of the Greek city-states, and the psychology of religion, for instance. But no topic is considered at sufficient depth. A line of argument will turn aside in a quite unpredictable way, and one may find difficulty in grasping the general direction in which one is being led. Some good points are made (on the strengths and limitations of systems theory, for instance), but the overall effect is confused. Kaplan fails to analyze the concepts he is considering ("justice," "alienation," etc.) precisely enough, and in political philosophy, conceptual precision, though not the last word, is certainly the first.

DAVID MILLER

University of East Anglia

Utopia and Revolution: On the Origins of a Metaphor, or Some Illustrations of the Problem of Political Temperament and Intellectual Climate and How Ideas, Ideals, and Ideologies Have Been Historically Related. By Melvin J. Lasky. (Chicago: University of Chicago Press, 1976. Pp. xiii + 726. $35.00.)

"What begins in undifferentiated emotion," Melvin Lasky writes, in passing, about Sukarno, "ends in unlimited rhetoric." This book demonstrates little more than the truth of his observation. Its organization is as uncertain as its audience; it has no clear thesis; and any arguments it does present emerge only with difficulty from an inchoate welter of quotations. These, distributed according to no discernible pattern among 16 chapters, are paraded laboriously along 700 wearying pages; of these, 100 are devoted to footnotes, many of which are as needlessly prolix and repetitive—and as badly proofread—as the text itself. As references, the footnotes are inexact and inadequate. To give nothing more than an author's last name in an inordinately long and unwieldy book without a bibliography is not Lasky's only act of enmity towards any reader ill-advised enough to try and use *Utopia and Revolution* as a guide to further reading—or even (lexicon-laden as the argument is) as an anthology of quotable quotes.

Lasky's technique is to bombard the reader with a battery of oddly juxtaposed quotations (some of which he soon repeats) and dropped names. But to what end? If this is evidence, what does it support? Lasky says nothing coherent or sustained about the causes or course of any revolution, although for some ultimately unspecified reason he considers their respective outcomes all to have followed much the same pattern. He regards his book as "a history of ideology." That few, I suspect, would agree does not deter Lasky from believing, apparently, that the kind of tenuous link he is able to establish between utopian, "ideological" thinking—together with its internal mental tension—and various revolutions will suffice to characterize them. Lasky blithely, unproblematically argues on the (unstated) assumption that revolutions can be treated as nothing more than the products of *intellectual* desperation—leaping fully armed, as it were, from the minds of utopians like Minerva from the brow of Jove. This he does on the basis of a silly and patronizing distinction between rebellion ("the work of the poor") and revolution ("the privilege of the middle classes") which he gets around to making in a footnote on p. 662. ("The revolutionary ... is a creature of fantasy.... For what is Revolution but the apocalyptic longing to create Paradise at a Stroke? ... an affliction of the modern mind ... a fevered dream of a storm and an earthquake that were never seen on land or sea?" The question is not unanswerable.)

As to utopia, Lasky consigns Paul Tillich's arguments (which he approves) to a footnote, as "metaphysical," "meta-theoretical." His own cannot be so described. They rarely rise above the level of the relentlessly middlebrow ("utopians love mankind too much, and ... revolutionaries loved men too little") and/or the soon-to-be-contradicted generalization ("all utopians are revolutionary [though not all revolutionaries are utopian]"; "the revolutionary is the wrong man at the wrong time. He comes too late—with too much. He calls for violent change when for the first time peaceful change seems possible. He insists on blood in the streets when ink on paper might well do").

All this platitude is meant to point to the conclusion that, whereas "revolutionism" is today "poor fare, bare bones from other feasts," moderation "moderately applied is reasonably unimpeachable"; and to "The English Ideology," its "sense of the gradual and the practical" as opposed to cross-channel extremism. Yet, even apart from the lameness and flatness of these guarded but banal recommendations, Lasky deals at length with the English Revolution, when Englishmen were least "temperate" and "pragmatic." To describe the Levellers, however, as "moderates" may be no more bizarre than to refer to Hazlitt's "kindred spirits from John Milton to Leon Trotsky"; or Giordano Bruno as "the 'missing link' between Dante and Condoret"; or Marx as one for whom "the longed-for means was the end; the revolution was itself utopia." (Lasky's point that the proletariat is a "collective Messiah" is much less original.)

In a vain attempt to counteract these infelicities, Lasky provides some interesting, but unoriginal and incidental, material—on Thomas More, on Sidney's *Arcadia*, on Bruno, on Milton, and on those (termed turncoats by less charitable commentators) who became disillusioned with revolution, particularly the slippery Marchamont Nedham and Sir James Mackintosh, whose early attack on Burke, *Vindicae Gallicae*, is indeed "the ablest ideological defence of the French Revolution," at least in English. These exceptions (worth reading, but carefully: the scholarship is uncertain) do not pull Lasky's book out of the morass into which his simplemindedness and indiscipline plunge it again and again—a morass akin to moderation as Kant once described it (which Lasky, in all innocence, quotes): "The 'Venetian treacle,' wherein so many good things have been mixed that it is good for nothing."

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The Politics of Decision-Making: Strategy, Cooperation, and Conflict. By Allan W. Lerner. (Beverly Hills, Calif.: Sage Publications, 1976. Pp. 215. $11.00, cloth; $6.00, paper.)

The analysis of scientific expertise in the policy process has long been impeded by a dearth of systematic conceptualizations which could effectively cue empirical research. Allan Lerner provides a conceptual framework for the interaction between "experts" and "politicos" within the context of full-time, career relationships in an institutionalized format. "Expertise" is defined in terms of subjective perceptions of both expert and non-expert within a substantive decision-making context. Conceptualizing expertise as a pattern of behavior rather than as an epistemologically defined commodity, this study offers a multilevel exposition of expert-politico interaction, within the negotiative process of a "decision-group" setting.

Conceptualization and analysis then focus on the variables which might shape the negotiating behavior of experts vis-à-vis politicos: chapters deal with role variables, variables of individual personality, problem content variables, organizational variables, and societal variables. Concepts of group interaction and of the social-psychological dynamics of role behavior constitute a primary thrust of analysis. In order to help identify the behavior patterns manifested in negotiations between experts and politicos, experiments were run on 50 subjects who were involved in experimentally created negotiative interactions of simulated expertise. The data were then subjected to factor analysis and statistical tests. This endeavor resulted not in "firm conclusions" about the behavioral orientations of experts and nonexperts, but, rather, in the illumination of fruitful lines of hypothesization and inquiry, such as the suggestions, consistent with the statistical analysis of the data, that "experts tend to generalize their expertise and expertise exaggerates assertiveness" (p. 81).

The analysis is notably effective in providing what seems to be a useful, epistemologically conscious, and heuristically valuable contribution toward a theory of expertise in public policy, and toward the development of hypotheses for empirical investigations into expert roles and interactions. While exhortations for theory building and for future hypothesizing are all too often pseudoscientific incantations whose real purpose is as a talisman for the researcher's empirical failures and epistemological inadequacies, Lerner actually provides something to work with, and makes a persuasive case that it is worth working with. Moreover, the epistemological consciousness of the work is not confined to closing caveats but is operationalized throughout, as the author carefully articulates the limitations of his methods, data, and conclusions and does so at a level of sophistication which in major part provides the endeavor with its heuristic utility and insight. The "basic conceptual tool kit" proffered is a carefully crafted and potentially useful set of conceptual baggage for future inquiry.

While the conceptual toolkit is constructed well enough to serve either theory builders or working researchers, its primary limitation (one which may indeed be inherent in the degree of conceptual integration and epistemological consciousness manifested) is in the scope and range of jobs for which it can be used.

The focus on in-house, full-time expertise neglects a most crucial dynamic in bringing specialized knowledge to bear upon policy decisions: the battles (political and epistemological) within scientific elites outside the formal, organizational context of the policy decision itself, as manifested in the ABM debate, the ozone layer controversy, and conflicts over the safety of nuclear power; and as manifested in the current proposal to establish a "Science Court" to settle conflicts of scientific expertise.

The definition of expertise employed by Lerner, rooted in the subjective perceptions of parties to a decision within an organizational context, obscures the crucial role of professional societies and learned communities in defining, certifying, and supplying expertise in areas of specialized knowledge, and, in addition, lumps the complex dimensions of *scientific* expertise (with its unique claims and status) with more bureaucratic and experiential forms of expertise. Though a discussion of broad social factors influencing expert-politico relationships is presented (Ch. 6), the special implications of what Robert Lane terms the "knowledgeable society" and the dynamics of the politics of science are neglected.

For the negotiative process between experts and policy makers is also shaped by the states of conflict and consensus among expert groupings and communities themselves, as is the expert role in a given policy context.

Still, it would constitute a kind of epistemological "catch-22" to fault this work for not biting off a bigger chunk of the phenomena with which it deals, for epistemological consciousness and precision are infinitely more valuable than a broad scope per se, which too often leads us into the vast empirical wasteland that C. Wright Mills so aptly described as the realm of "grand theory."

This careful work, rich in bibliographical sources, is a significant conceptual contribution to the literature of public policy, and, as an added treat, it is lucidly and engagingly written.

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The Leadership Passion: A Psychology of Ideology. By David Loye. (San Francisco: Jossey-Bass, 1977. Pp. 249. $12.95.)

It is the odd book in this era of pragmatic specialization that sweeps through the theoretical views of Hegel, Nietzsche, Marx, Pareto, Weber, Freud, and Pavlov. It is stranger still when in the same book, written by a self-professed "social scientist," the psychological and sociological studies of Adorno, Rokeach, Eysenck, and Tomkins are explored. David Loye, present director of the Program on Psychological Adaptation and the Future at UCLA, is to be commended in his choice of both a wide-ranging and challenging topic.

The purpose of *The Leadership Passion* is to "regain a lost sense of ideological bearings during a time of great psychological and sociological change" (p. x). To accomplish this end, Loye surveys a large body of literature in both the philosophical and psychological traditions. His conclusion holds that we are entering a new leadership period which he calls "the third major shift in basic ideological style in the history of humanity" (p. xii). This "middle" style is the result of the gap between psychological man and sociological man. For Loye, the first thousands of years in man's history were conservative (i.e., the cave man mentality). The second period of liberalism, only a few hundred years old, witnessed the rise of the individual. Now we are entering this dramatic new age which Loye disappointly dubs, "Beyond the 'End of Ideology.' "

The book is broken into five neat parts. The first introduces his study and discusses the theoretical material on "Left and Right." Part 2 surveys the vast gamut of writings on the measurement of ideology. In the two middle parts Loye presents his own psychological studies on the ideological personality. These chapters are oriented toward the social scientist interested in design, methods, and results. Chapters 5 and 6 are noteworthy because they are eloquently written case studies of two campus leaders in the Princeton University study on which Loye bases his conclusions (see pp. 77–96). The concluding part of this seemingly disjointed effort is a highly original and suggestive series of implications for social scientists, therapists, leaders, and managers in this emerging "middle" period in the history of humanity.

Loye's findings, by way of psychological testing and interviews replicated on two different types of populations, inform the conclusions drawn in the final chapters. A new model of ideological functioning is provided based on norm-changing versus norm-maintaining (see Ch. 12). This dialectical theory, to be released in a more elaborate companion book *The Psychology of the Middle,* goes beyond earlier theories extending on the contributions made by Rokeach and Tomkins.

As with most good books, *The Leadership Passion* raises as many questions as it answers. Many hard-nosed social scientists will not appreciate the rambling chapters discussing the philosophical background to this delicate subject. Political theorists, on the other hand, may find the empirical hypotheses which Loye develops—concerning norm-changing versus norm-maintaining, activism-inactivism, extremism-moderation, risk taking, Machiavellianism, alienation, anomie, locus of control, and parent-child variables—a bit overdrawn, if not misguided.

There are inherent problems with the use of psychological testing that are multiplied when the researcher draws speculative implications that seem unwarranted. Any transformation from Pareto's "Lion and Fox" to Rokeach's model as an "Irreality-Reality Relationship" is at the very last tenuous. Nevertheless, Loye deserves attention as an innovative synthesizer who is simultaneously both more than a behavioral psychologist and less than a political philosopher. Whether Maslow and Hegel can be resolved, even "at the level of self-actualizing," *is* an entertaining question. We can thank Loye for calling intriguing issues to our attention.

THEODORE R. MALLOCH

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Ambiguity and Choice in Organizations. By James G. March and Johan P. Olsen et al. (Bergen, Norway: Universitetsforlaget, 1976. Pp. 408. $20.00.)

The article, "A Garbage Can Model of Organizational Choice," by Michael Cohen, James March, and Johan Olsen (*Administrative Science Quarterly,* 17 [March 1972], 1–25), was an exciting addition to the literature on decision making in organizations. It focused on "organized anarchies" (loosely coupled systems characterized by ambiguity in goals, technology, and participation), and it presented in verbal and computer simulation form a model of how decisions tend to be made in such structures. In the garbage-can model, whenever a decision is to be made, an assortment of problems, solutions, and participants flock to it and attach themselves to the proceedings. This usually creates such a load on the choice process that it cannot be completed by resolving the burden of issues. It is generally completed only after all but a few of the issues have wandered off in search of other, more promising choice situations. These propositions were discussed further in Cohen's and March's *Leadership and Ambiguity: The American College President* (New York: McGraw-Hill, 1974), and the current book is a third installment in the series.

This book elaborates constructively on the themes of ambiguity and choice but it does not attempt to integrate the various themes and subthemes, nor does it attempt to bring the major ideas to a stage of detailed completeness and theoretical maturity. The aim is more modest: "Without attempting a complete reconstruction of ideas about decisions in organizations, we will try to outline some possible perspectives that may make ordinary experience in organizations somewhat more explicable" (pp. 10–11). The individual chapters do contain, both implicitly and explicitly, many ideas and findings that should cause some rethinking and further development of the original framework. I would hope for a fourth installment in this series that would accomplish just that. In the meanwhile, *Ambiguity and Choice in Organizations* leaves us with the opportunity to do our own rethinking and with a provocative collection of data and theoretical discussion on which to base it. Functionally speaking, the important achievement of the book lies not quite so much in theoretical progress as in conceptual clarification. It breathes vitality and reality into the ideas of the model by transferring them from the computer to the organization.

Much of the book is written by James March, who contributed to seven of the seventeen chapters, and Olsen, who contributed to eight. March has long been a leading thinker in the field. Johan Olsen (University of Bergen, Norway) is little known in America, but particularly through four chapters of sole authorship, he emerges here as an organization theorist of impressive stature in his own right. The other authors, many of whom are young Scandinavian scholars, contribute quantitatively less, but I found each chapter, without exception, to be well executed, creative, and valuable. It must be noted that execution would have been considerably enhanced by entrusting the whole to an assiduous editor.

Seven of the chapters are about ambiguity. Four of these have to do mainly with learning, two with attention and participation, and one with power. Since there is little detailed effort to relate the ideas to one another, the various types of ambiguity must stand more or less alone. This by no means destroys their value. It is quite a contribution just to make us forcefully aware that these sorts of ambiguity do exist in organizations. It is perhaps somewhat more serious that the ideas on ambiguity are for the most part not connected by general theoretical statements to processes of choice. It is probably true that without ambiguity we would not find much garbage-can decision making in organizations, but in the presence of ambiguity we seem to find all sorts of decision making, not just the garbage-can process. If ambiguity in goals, technology, learning, and attention do indeed have a special impact on the tendency toward garbage-can decision processes, it would be well to have hypotheses that specify how and under what conditions.

The other ten chapters are about choice. One restates the general theory of garbage-can decision making, six are case studies and analyses that illustrate the model in action, one presents a theoretical perspective on areas of choice in which individual decisions tend *not* to become garbage cans, and two exemplify the latter with several additional case studies. Much of the importance of the book lies in its collection of case studies. It is significant that whereas the original garbage-can model emphasized the organizational level of analysis, this book emphasizes single instances of choice. This distinction merits attention in future research. Originally, the garbage-can model referred primarily to a structural/processual framework in which a multiplicity of decisions are made simultaneously and in series. The important concepts of segmentation, problem activity, problem latency, and decision difficulty (pp. 27–34) refer to whole organizations as a unit of analysis. In the present book, the term "garbage can" is used much more commonly to refer to a single process of choice—to its tendency to become a temporary receptacle for wandering problems, solutions, and participants, regardless of how other decisions in the same organization might be made. The individual process of choice is "black boxed" in the original article but repeatedly exemplified (without actually modeling it) in the current book.

In this light, the attention of the reader gravitates toward possible explanations for why certain individual decisions tend to become garbage cans. Some degree of explanation is afforded by the three primary characteristics of context originally stipulated by Cohen, March, and Olsen—unclear goals, uncertain technology, and fluid participation—but in themselves these will probably not carry as very far toward explaining variance in the extent to which any single decision in any organization might become a "garbage can." Unfortunately, no chapter is devoted primarily to developing such an explanation more fully, but much may be gleaned from a comparison of the case studies in which choice opportunities did and did not become garbage cans. One significant line of explanation is suggested by Rommetveit in his analysis of the establishment of a third medical school in Norway (Ch. 7). Garbage cans appear to be stimulated by a destabilization of the prevailing value structure, i.e., by a situation in which the dominant preference ordering begins to crumble and there is movement toward a new weighting of goals and values. Star billing seems to go to the kind of ambiguity featured in times of transition and reappraisal. Several of the case studies appear to exemplify this point. One important finding is that under these circumstances, the primary purpose of certain decision processes is not to produce a specific outcome, but rather, through the airing or "exercising" of problems, to maintain or change the organization as a social unit. A second explanatory theme might be called, in the argot of the garbage-can model, "load potential." It has to do with the situational potential of a choice opportunity for attracting troublesome problems. How much time is available for making this focal choice? How important is the choice itself to participants? How many other attractive choice opportunities are around? How relevant is the focal choice to the collection of problems highlighted by a current destabilization of values or self-image? Perhaps other explanatory ideas are to be found from a careful reading of the various chapters; these two appear to stand out.

Lastly, many of the examples in the book are not truly instances of organizational behavior, but rather of the common sort of multi-organizational behavior that goes into the making of public policy. Remarkably similar examples can be found over and over again in the case-study literature on public administration and policy. It is small wonder that public policy making tends to be full of garbage cans, since complex societies (in contrast with industrial firms, for example) are endemically in a state of ambiguity or instability of value structure, perhaps even more so in this century than ever before. Many of the remaining examples in the book are taken from universities. I do not believe that universities have always been significantly characterized by garbage-can decision processes, nor will they necessarily continue to be, but surely in recent decades they have been undergoing profound, destabilizing institutional change as an industry and have also been a focal point for the expression of value change in the surrounding culture. Contemporary universities and polities, at least in the West, are therefore two kinds of social system in which one can expect to find a large portion of garbage-can decision making. The original garbage-can model, a process model at the system level, is therefore particularly important for universities, for the making of public policy, and for similar "organized anarchies." At the level of the individual process of choice, garbage cans can crop up anywhere, and in this sense the contents of the book are of importance for understanding some amount of significant behavior in nearly all organizations.

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Social Research in Conflict with Law and Ethics. Edited by Paul Nejelski. (Cambridge, Mass.: Ballinger, 1976. Pp. xiii + 197. $15.00.)

As many political scientists know from experience, there is a growing movement towards the regulation of research in the United States as well as in other countries. One of the important forms of this movement is the development of institutional review boards in universities and other institutions that receive federal grants. These boards are responsible for carrying out provisions designed to protect human subjects in the course of research when such subjects are deemed to be "at risk."

This regulatory movement is the result of conflicts between the values of research, and other values such as individual privacy. Some of these conflicts have been dramatic. The syphilis experiments conducted in the South over a period of decades in this century, and experiments involving the sterilization of medical subjects, suggest the intensity of value conflicts that can occur.

One of the more important conflicts centers on confidentiality in research. The case of

Richard of Rockford, Inc. v. *Pacific Gas and Electric Company,* decided by United States District Judge Charles B. Renfrew in San Francisco on May 20, 1976, illustrates the problem. This was a civil action in which the plaintiff, an equipment manufacturer, sought damages for breach of contract from Pacific Gas and Electric Company in California. The primary issue was whether the plaintiff had properly installed certain equipment. Under a pledge of confidentiality, Mark J. Roberts of the Harvard School of Public Health and the Kennedy School of Public Affairs had interviewed employees of Pacific Gas and Electric Company as part of a research project investigating the manner in which utilities make environmental decisions. The plaintiff wanted to know the identity of the Pacific Gas and Electric Company employees interviewed by Roberts and the substance of what they said. The court refused to grant a motion to compel Roberts or his research assistant to testify. The court held that the information sought was available from other sources and did not go to the heart of the dispute.

In this case there was a conflict between the promise of confidentiality extended by Roberts to those he interviewed and the right of the plaintiff to compel disclosure of information that might contribute to a fair resolution of the dispute. The court held, in these particular circumstances, that the value of confidentiality outweighed the potential value of disclosure.

Social Research in Conflict With Law and Ethics is a valuable compilation of articles and materials on the question of confidentiality in research and related questions. Many of the materials published in this volume were prepared for a conference of 25 Germans and 10 Americans held at the University of Bielefeld in March, 1974. The conference explored the rights and responsibilities of researchers concerning confidentiality and related matters.

The book consists of three sections. The first is a summary of the discussion at Bielefeld, by Paul Nejelski, and an analysis of the major dimensions of confidentiality and research problems. The second section consists of ten papers prepared for the conference, several of which analyze the nature of conflicts between the values of social research and other values, while others discuss such issues as national security and privacy. The third section is directed to methods of avoiding and resolving confidentiality problems. This final section includes a proposed researcher's shield statute drafted by Paul Nejelski and Howard Peyser.

This is a valuable compilation of materials, ably introduced and organized by the editor. This book is indispensable to those concerned with legal and ethical dimensions of social research.

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Epicurean Political Philosophy: The De Rerum Natura of Lucretius. By James H. Nichols, Jr. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 214. $11.50.)

James Nichols' *Epicurean Political Philosophy* is intended to illuminate the commonly neglected philosophic tradition begun by Epicurus and mainly preserved in the *De Rerum Natura* of Epicurus' famous Roman follower, Lucretius. Nichols demonstrates that Lucretius' poetic restatement of Epicurean philosophy must be treated seriously by students of political philosophy, for it constitutes an "abiding alternative" to both the Socratic and modern political philosophy and is especially important in that it shares the view of nature held by those who founded modern science and philosophy yet issues in neither the now "dissipated" hopes of the early modern philosophers nor the nihilistic despair of their descendants (p. 180).

Against those who doubt whether Lucretius furnishes a political philosophy rather than the final statement of Democritian physics, Nichols shows that the teaching of *De Rerum Natura* primarily concerns the best human life; the account of nature is needed not for its own sake but to free us finally from the fears that undermine human happiness. For Lucretius, "ethics takes precedence over physics" (p. 15). Though he ignores the question of regimes, Lucretius does present "a philosophy of politics" that includes an account of the origin and significance of political life. Though he depreciates political life, he does not in this wholly depart from the Socratic view; further, the rhetoric through which Lucretius accomplishes this depreciation and generally guides the souls of his readers presupposes a comprehensive political knowledge. Indeed, according to Nichols, Lucretius presents his teaching in poetic form despite Epicurus' own disdain for poetry because only thus could he hope to lead his reader through an appreciation of what is "sweet" in the Epicurean view—freedom from the fear of divine wrath here or in an afterlife—to a sober acceptance of its ultimate sadness—the recognition of "an infinite nature indifferent to human needs and purposes." Finally, religious belief, the subject of Lucretious' analysis and polemic, is inextricably connected to political society, as Nichols demonstrates in his careful treatment of the Lucretian account of "our world." As language emerges in connection with prepolitical society, so religion comes into being with political society as a more complex fear of death arises therein. The distinctively political pursuit of wealth and honor, like religion, is a futile attempt by human beings to flee our awareness that we are mortal and alone in an equally mortal world; both lessen human happiness by introducing the fear of punishment. Epicurean philosophy constitutes the only adequate response to this awareness, albeit a response beyond the reach of most.

Along with Leo Strauss' "Thoughts on Lucretius," in *Liberalism Ancient and Modern* (New York: Basic Books, 1968), which has clearly been an important guide for Nichols, the present work may serve students of political philosophy by advancing the careful study of a writing important both as a relentless analysis of the human situation and as an alternative to the better-known forms of political philosophy, in the light of which those other forms may obtain new clarity. Nichols engages in the comparative inquiry directed towards this clarification in the concluding chapter of his book. There he contrasts the teaching of Lucretius with that of Hobbes, Montesquieu, and Rousseau. As the magnitude of this task becomes apparent precisely on the basis of Nichols' presentation of the Lucretian alternative, it should be no serious complaint if the reader is struck rather by the questions raised in this comparison than by the solutions furnished. If Hobbes shares the Lucretian view of nature, denies with Lucretius that human beings are by nature political, and attaches a similar significance to fear, one must ask with Nichols why Hobbes, unlike Lucretius, urges a bold new human enterprise based upon the unlimited conquest of nature and his own political science. Nichols' suggestion that the difference derives from the "primacy of politics" for Hobbes and Hobbes' acceptance of the vulgar view of happiness (p. 183) seems insufficient when one recalls that Hobbes, no less than Lucretius, viewed the motive animating political life as illusory (see, e.g., *De Cive* 2.10.9) and reproached Aristotelian political science for its reliance upon common opinion. This is not to deny Nichols' observation that Hobbes differs with Lucretius in what he expects "from politics," as in his egalitarianism, nor to question the utility of Nichols' comparison, but to suggest the need of further search for the explanation of this difference. Might the inquiry here, as in the comparisons with Montesquieu and Rousseau, become more revealing if the discussion were extended to include the classical alternative, a political philosophy that regarded the political community and justice as natural if ultimately transcended by friendship and philosophy, and within which political life received its most sympathetic treatment if also its final depreciation?

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Conservatism. By N. K. O'Sullivan. (New York: St. Martin's Press, 1976. Pp. 173. $10.95, cloth; $4.50, paper.)

O'Sullivan does not attempt an "exhaustive study of conservative practice . . . or thought"; rather he pursues "an examination and critique of the three principal kinds of arguments against radical change [which have been] derived by conservative thinkers from different conceptions of imperfection" (p. 5). In this enterprise the author achieves moderate success. His basic typology in the opening chapter is thought-provoking, and his second and third chapters (on French and German conservatism) are the best brief introductions that I have seen. But his chapter on British conservatism suffers by comparison, and his final chapter on the crisis of conservative ideology in the twentieth century reveals significant lacunae.

The British don defines conservatism in its essence as a philosophy of imperfection. The three sorts of views which project this inevitable imperfection are, in O'Sullivan's typology, these: (1) the view that this "imperfection of man's condition is derived from a moral or theological vision of the world . . . as an ordered, hierarchical whole" (Burke, de Maistre) (p. 22); (2) the view which sees "the source of man's imperfection . . . within the flux of the historical order itself" (German romantic conservatism, Carlyle, Coleridge) (p. 23); and (3) the view which sees man's imperfection as essentially tragic in that the "world precludes the fulfillment of men's loftiest dreams because not all the goods that they desire are compatible with one another" (Hume, Constant, Tocqueville) (p. 27).

The description of French conservatism includes excellent discussions of de Maistre and Bonald as defenders of Christendom against the onslaughts of democracy rampant. This reactionary view (an extreme form of typology one) interprets hierarchical order as compatible only with the traditional monarchical and ecclesiastical orders. It leads ultimately to the support of corporatism and the *Action Française* when the restoration of a medieval social order appeared no longer viable. The more moderate French view, O'Sullivan argues, is that of Constant and Tocqueville, who (exhibiting typology three) were willing to come to terms with post-revolutionary France.

German conservatism was, in contrast, characterized by the romantic view of typology two. This romanticism with its emphasis on the flux of history tended towards a glorification of power, and ultimately the state, quite at odds with English, French, and, for that matter, American conservatism. This theory of the peculiarity of the German conservative tradition is intriguing in that it offers a plausible explanation of the intellectual origins of German fascism.

Both French and German conservatives failed "to formulate a coherent conservative ideology" which could serve as the basis for political action (p. 82). The strength of British conservatism, in contrast, consisted precisely in this success for Disraeli and Lord Randolph Churchill were able to expound an ideology which served as the foundation for a popular conservative party which has endured until the present day. O'Sullivan's emphasis on the conservative politics of the second half of the nineteenth century results, unfortunately, in an inadequate treatment of Burke. For this I fault him.

The final chapter is the least satisfactory, particularly in its treatment of American conservatism. While there is, for example, a good analysis of Strauss and Voegelin, and of their similarities to Simon and Maritain (pp. 132–34), certain important aspects of American conservatism—the Nashville Agragians and Richard Weaver—are simply missing. The author, in part, reiterates the conventional liberal critique of the new conservatism as being unrealistic within the fundamentally liberal American tradition; he has forgotten Willmoore Kendall's attempt to construct a genuinely American conservative affirmation based on his notion of the virtuous people and Harry Jaffa's interpretation of Lincoln as the exemplar for American conservatives. Another unexplicable weakness is O'Sullivan's treatment of the conflict between traditionalists like Kirk and the neo-liberals like Hayek, von Mises, Roepke, and Friedman. He ignores attempts by conservative intellectuals like Frank Meyer to fuse these two schools in an intellectually consistent whole. Finally, O'Sullivan argues that the new conservatives have not articulated a genuine popular appeal, disregarding the proposals of Kevin Phillip's *Mediacracy* and William Rusher's *The Making of the New Majority Party*.

In sum, O'Sullivan has, given his space restrictions, partly succeeded and partly failed. His book might be best described as a useful prolegomenon to Russell Kirk's *The Conservative Mind*, a sweeping history of Anglo-American conservative thought, and George Nash's *The Conservative Intellectual Movement in America Since 1945*, a brilliant and thorough history of the resurgence of conservatism in postwar America. For without works like the latter two, one would be ignorant of far too much of the richness of the conservative tradition on this side of the English Channel and the Atlantic Ocean.

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Karl Marx and World Literature. By S. S. Prawer. (New York and London: Oxford University Press, 1976. Pp. xii + 446. $19.50.)

Despite a large and growing number of studies on Marx's views on literature and art and on Marxist aesthetics (see, for example, Lee Baxandall, *Marxism and Aesthetics: A Selected Annotated Bibliography*, New York: Humanities Press, 1968), it should by now be clear that there is no fully articulated Marxian aesthetic. On the other hand, there is an aesthetic *dimension* to Marx's thought, one which is operative both in his use of language and in his mode of conceptualization. That is, apart from his expressed views on art in general and on such specific works as Eugène Sue's *Les Mystères de Paris* or Ferdinand Lassalle's *Franz von Sickingen*, Marx's underlying conceptions of the relation of art to reality integrally condition his critiques of capitalist society and bourgeois social theory. The study of this unfolding dimension of his thought is therefore inseparable from that of the thought as a whole. And, within the complex structure of the critique, the aesthetic concepts often mediate between what to many have appeared as discontinuous "moral" judgments and "analytical" conclusions. Further, while it is indeed necessary to discriminate between the range and quality of Marx's work and that of other Marxists, the same interpenetration of aesthetic with other categories is characteristic of much of the best work of the latter. Steven Marcus, for one, has recently reinforced the cognitive claims of literature and literary criticism in his valuable study of Engels' *The Condition of the Working Class in England in 1844*, as much a "work of art" as a masterpiece of social criticism (*Engels, Manchester, and the Working Class*, New York: Random House, 1974); and similar explorations of the work of theorists like Antonio Gramsci and Herbert Marcuse either have been or might well be undertaken.

The presence and importance of this aesthetic dimension in the work of Marx and some Marxists is, of course, *not confined to* Marxism but reflects its formation within and obligation to classical bourgeois culture and, above all, to German Idealist philosophy. So significant in this aspect of Marxism that one is tempted to second Lenin's well-known observations on the relation of Hegel's *Science of Logic* to the "logic of *Capital*" with parallel claims concerning the value of Lenin's *Aesthetic* for the comprehension of Marx's "poetry of commodities." Already in the 1930s the Soviet aesthetician Mikhail Lifshitz had underscored the validity of such claims. However, it would be no exaggeration to insist that the bulk of *Marxschriften,* including some notable contributions, has displayed relative insensitivity to the aesthetic mediation of concepts in Marx. No wonder: for, as Marcuse has pointed out, it is precisely the late capitalist liquidation of the multidimensional culture of the "liberalistic era" which has rendered the aesthetic, philosophical, and other aspects of Marx's *oeuvre* nearly invisible to friendly and hostile critics alike.

In this context one can best assess the considerable merits of S. S. Prawer's recent study. Invocation of the aesthetic mediation in Marx is one thing; its detailed reconstruction is another. The latter will require immersion in empirical and textual detail and will be the work of many hands. Prawer characterizes *Marxism and World Literature* as a "modest contribution" to this project; however, his "chronological scrutiny of Marx's dealings with literature," employing "full quotation, in English, from the entire corpus of Marx's work" (p. vii), has put all future students of this subject in his debt and has resulted in a volume that, in its own way, deserves to be placed in the company of some of the more notable recent contributions toward the understanding of Karl Marx.

Marxism and World Literature is, in the first place, a scholarly Baedeker to the literary and aesthetic components of Marx's thought, from his early schoolboy exercises and unsuccessful experiments in verse to his late involvement with Slavic studies. Yet it is no mere compilation of "references" or "metaphors," but follows Lifshitz in attempting to situate the vast range of such "references," compressed or extended, within the totality of Marx's intellectual trajectory. While it is not possible to summarize so richly illustrated a work, and while some will not agree with all the interpretations offered, it will no longer be quite so easy for writers on Marx to ignore, glide over, or repress the aesthetic dimension so skillfully evoked by Prawer. Particularly notable in light of recent tendencies to dismiss him as a narrow theorist of "instrumental action" is Marx's conception of literature, emphasized by Prawer, as an essential mode of human *"self-constitution"* (p. 404). Although not himself uncritical, Prawer has few polemical knives to sharpen; and *Marxism and World Literature* will lend little comfort to such neo-reductionistic interpretations.

THEODORE MILLS NORTON

Vassar College

Social Science and Public Policy. By Martin Rein. (New York: Penguin, 1976. Pp. 268. $2.95, paper.)

This series of essays is a distinct contribution to definition and self-consciousness in the currently popular but extraordinarily mushy "field" of public policy. The cumulative effect is to identify and explore some hitherto-unexamined but absolutely central questions or dilemmas of social policy analysis in a most thoughtful and provocative manner. Much to his credit, Rein proceeds to offer solutions that, while not equal to the questions raised, are nevertheless very important and potentially rewarding in their own right.

Rein's essays are intended for an audience already committed to making recommendations to policy makers, or to some other form of frank acceptance of a role for values in policy analysis. A secondary audience would be social scientists who recognize at least some of the problems involved in employing an empirical-positivist social science (based on natural science models) for public policy studies. But many others could profit greatly from this collection, if their commitments to pure-science orthodoxies did not prevent it. And, even if taken only as a reference source for some published and many unpublished social policy studies of the late 1960s and early 1970s, the book would have substantial value for teaching purposes.

Rein's basic thesis, articulated in different ways in different essays, is that a proper and explicit role for values must be found in any approach to social policy analysis. This task is enormously complicated by the fact that the act of research itself inevitably involves applications of (sometimes unrecognized) values and ideology. For example, "facts" are not totally distinct from but are rather at least partial extensions of the values that lie implicit in the conceptual frameworks, language, ideology, premises, and methods of social "science" as it is presently grounded. Rein shows in a variety of concrete illustrations why values cannot be excluded from either the policy research or analysis process *or* recommendations, and argues persuasively that their role must be explicitly acknowledged rather than ignored, minimized, or defined away as not part of appropriately scientific research practices. (He does not mean that the fact-value distinction so important to positivist social science is illusory *only* in policy studies—its nonexistence is just inescapably clear in that context, the one with which he happens to be concerned.)

Rein's search for ways to explicitly integrate values into policy research and recommendation leads him to make three major suggestions. The first is the more comprehensive, a call for a "value-critical" approach that would see facts, values, and theory as inseparably related to each other, would make value premises themselves (particularly one's own) subjects of inquiry, looking specially at their consequences, and would see facts only in terms of their corresponding value assumptions. Related to this general approach is the concept of "storytelling," in which metaphors and analogies are woven together in the form of a story that extends the experience of the past into a trustworthy pattern of recommendations for the future—thus avoiding the great difficulties created for positivist policy analysis methods when "research" is asked to confront the future. Also related is the suggestion that multiple research frameworks ("paradigms") be employed on given problems so that insights from each (i.e., each set of value premises applied to the description and interpretation of "reality") can be used to understand possible alternatives and their consequences.

Rein ends up acknowledging the ambivalence created by recognizing that social science offers no basis for choice between competing frameworks for social research and understanding, and simultaneously feeling the need to further morally justifiable and politically feasible goals. He sees no hope in "value-committed" approaches, such as Marxism (which he also sees as another form of deciding on values as a result of objective facts, essentially indistinguishable from the positivism he rejects). His solution is to proceed skeptically, testing each of the multiple paradigms' conclusions/interpretations against observable reality, always analyzing policy broadly as the interaction between (a) values/purposes, (b) operating or implementing principles, and (c) outcomes for people and problems. Essentially, he remains a pragmatic empiricist, aware of the limitations of his tools yet determined to apply reason to the amelioration of social problems as fully as possible, particularly in behalf of his central value, equality.

Although the book suffers somewhat from the duplication and lack of coherent development in a collection of separately written essays (chapter 2, for example, appears more advanced and more synthetic than any of the others), it still must be placed among the leading contributions to a field sorely in need of conceptual and methodological self-consciousness. For too long, we have been stuck on questions ("Should social scientists make policy recommendations? Is this policy research truly scientific?") that *assume* the validity of the very premises of positivist social science and its associated social order that we should be challenging. With Rein's careful analysis and provocation, we may begin to ask what sort of "science" we have, and what kind we would need to integrate understanding of our past and present with programs of social reconstruction appropriate to solving our problems and creating a better future.

KENNETH M. DOLBEARE

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Revolutions and Revolutionaries: Four Theories. By Barbara Salert. (New York: Elsevier, 1976. Pp. ix + 161. $9.50.)

The simplest way to review this book would be to plagiarize from the flap which asserts that "the author is one of the few political scientists qualified to. . . ," that the book "cuts through the complex theories. . . ," and it is "an extraordinary work: well written and clearly organized—marked by exceptionally precise, lucid language and rigorous reasoning." All of this is true. The book constitutes a metatheoretically self-conscious examination of four "theories of revolution": those of Olson, Gurr, Johnson, and Marx. It is short, precise, lucid, and rigorous. Salert's comments on Olson refer to classical articles from economic journals; her analysis of Gurr's theory is based on recent thinking and knowledge of psychologists concerned with aggression; her Marxist reading is broad and contemporary; and her philosophical credentials are impeccable. She develops arguments with a rare sense for reasoning. It is an "if, then" book, in which the "ifs" are treated seriously. Perhaps above all else this is simply an intelligent book.

As the generation of the graduate students of the sixties becomes domiciled in academia and as the prospects for revolution become a dim memory, books about revolutions proliferate. Some are politically passionate, some are intellectually sterile. Salert's is intellectually exciting and politically "objective." She separates her values from "the" facts; her scalpels are logical and empirical: a good theory, she argues, is one that is consistent and accounts for what we know to be true better than others. Her metatheoretical position is subtle, and she has an excellent sense for identifying the facts for which various theories must account. Her critique of rational choice theory is principally empirical: if free riders are so ubiquitous, where did they hide during mass revolutions, such as the French or the Russian one? The critique of Gurr is principally metatheoretical: a list of all possible causes, Salert argues, is not much of a theory. Theories must elucidate "proximate causes"; they must exclude as well as include. Her critique of functionalism, as represented nearly exclusively by Chalmers Johnson, is logical. With Marxism, problems become more complex since it is a theory that cites proximate causes and is falsifiable in principle, but only if the theoretical time of contradictions becomes related to the historical time; and this has never been done.

And yet in my view this is a misconstrued project. Perhaps the most obvious reason is that the four essays do not add up to anything other than "the state of the literature," and the literature is in a poor state. They do not add up to any knowledge about revolutions because this very concept assumes meanings within the particular theories that are so divergent that the bond among them remains purely nominal. Salert is well aware of the difficulty. She observes that "revolution" is not a datum but a construct: no one has ever seen a "revolution," but only a series of concrete events that become constructed as revolutions within the terms of a particular theory. Yet she proceeds to write about "revolution," although Olson's analysis of the free rider problem is a theory of collective action but not specifically and perhaps precisely not of revolution, Gurr's theory is intended to account for the variance of a three-dimensional continuous variable, Johnson's revolution is at best an ambivalent term, and for Marx revolution is a highly discrete event that occurs rarely and is easily recognizable as such. What collective action has to do with turmoil, and either with socialist revolution remains unclear, and the project falls apart at the seams.

Revolutions are not a pleasant phenomenon to deal with in terms of positivist epistemology. There have simply not been enough of them, and the only way to force them into the dogma of empiricist science is to sterilize all interesting questions by transforming the problem into one of "collective violence" in general, "collective action" in general, etc. There is no reason to expect that any scientific laws would be true of revolutions in general. Revolutions against particular systems of social organization are precisely that: revolutions against particular systems. They are shaped by the very conditions against which they are rebelling. What constitutes relative deprivation in one social system may not in another; what constitutes benefits and costs in one type of society may not in another. The very construction of "revolution" as an objective datum to be accounted for by an impartial, ahistorical observer leads to the impossibility of saying something reasonable about it.

Perhaps for this reason it is not surprising that most interesting things about revolutions were written not by positivist observers but by revolutionaries themselves: potential, actual, and frustrated. It is symptomatic that Salert chose to interpret the Marx of *Capital*—the theorist of the long run—and not the Marx of political writings. She says explicitly that the interpretation she adopts "relies on the positivist conception of Marxism" (p. 98). Yet the position which asserts that one can objectively account for the conditions which bring about a revolution is itself a theory of revolutions, for it denies implicitly that revolutions may have been potential under many circumstances under which they did not occur, precisely because people expected that it would result from "conditions," not from their actions.

These are disagreements, and they should not be misconstrued as criticisms. Within the terms in which the project is constructed, this is frequently a brilliant book. The essay on rational choice theory has particularly wide implications. Not only does Salert suggest that revolutionary behavior may be precisely the behavior which represents a breakdown of the self-interested rationality postulated by the theory, but she enters into a penetrating analysis of the relation between game theory and price theory and arrives at some conclusions that merit general attention and discussion.

I would like to recommend *Revolutions and Revolutionaries* not only because it is a book that as few others contributes to our enlightenment, but also because it would be an excellent textbook. If we insist that students become familiar with "the literature," we should at least provide them with good guides, and Salert's is the best of the genre.

ADAM PRZEWORSKI

University of Chicago

The Policy Vacuum: Toward a More Professional Political Science. By Robert N. Spadaro, Thomas R. Dye, Robert T. Golembiewski, Murray S. Stedman, and L. Harman Zeigler. (Lexington, Mass.: Lexington Books, D.C. Heath, 1975. Pp. x + 215. $17.00.)

When I received this collection of essays I was quite excited about reading them. I share the concern of the book about the meaning of public policy studies and their methodology. This prestigious lineup of authors seemed to offer valuable sources of insight. However after reading the work, I was disappointed; it is a case of tremendous promise and little performance.

Spadaro sets the basic tone of the work in his opening article which gamely attempts to put the study of public policy into a perspective vis-à-vis political science and then attempts to develop a new model to explain the decision-making process in the policy arena. The result is the weakest piece in the work. The "bold" new model is discussed for one and three-fourths pages. There is no conceptual definition, no rigorous exposition of logic, no operationalizing of terms and no attempt at theoretical integration to the field in general. It is as if the methodological advances of the last 20 years had been forgotten or ignored, leaving us with a bare-bones discussion of a model entitled the "quantum dynamics policy model." To add insult to injury, Spadaro then spends nearly as much space with a sophomoric discussion of the difference between nominal, ordinal and interval data. Incredible! Given the price of the book, the integrity of the publisher and the reputations of the authors, I had hoped for something more meaningful.

There are, of course, a few bright spots in the book, though one must look closely to find them. Dye's article on the challenge of public policy to political science offers a decent review of the literature but little insight. He does add one critical statement at the end: "Policy analysis is political science" (p. 53). If he had written this one line as his entire essay it would have been more stimulating. Yet the statement is troublesome to me because, as I recall, the focus on policy sciences stemmed from a desire to deal with policies as independent rather than dependent variables. Dye's statement indicates that this difference in focus has been lost as policy science swallows up political science, to paraphrase Herbert Simon.

Robert Golembiewski's article looks at the development of public administration theory and the study of policy and attempts to integrate the two. This is the strongest, most innovative and most insightful piece in the collection; Golembiewski should expand this effort into a full book. In a fascinating section the author analyzes the impacts of the emphasis on policy science on the content of the study of public administration.

Stedman's article on the relationship between political parties and public policy breaks no new ground. He simply spends his time asking familiar questions translated into the focus of a student of political parties. His summary shows the narrowness of traditionally oriented political scientists when he states:

> In summary, then, there is unlikely to be—to a great extent—the development of a network of organic relationships between departments of political science on the one hand and policy science institutes, centers, or laboratories on the other hand (p. 168).

There appears to be little evidence to support this strong statement. If it can be supported, this situation would, in my mind, further condemn political scientists to spending their time muttering to themselves in their offices and talking to each other at national conferences that are exercises in ego-massaging and jargon trading.

The final piece by Zeigler and Boss visits familiar ground in discussing the role of pressure groups in education policy. The work is a well-designed and executed research effort that moves beyond the ubiquitous case study that is the norm of policy studies, excluding the work of Dye, Sharkansky, Hofferbert, Ostrom and Caputo. But the fact remains that the piece is still traditional political science. (The reader may define that term in any manner that is comfortable.) If one were to substitute the word "decision" wherever one finds the word "policy" in all of these articles, two things would occur. First, the essential meaning would not be changed at all. Second, the articles would not qualify as articles about public policy. Thus the criteria to be met for a study to be considered public policy analysis seem quite scant and suspect. Perhaps I have overstated this case for the sake of impact but I feel this is a real problem in the study of public policy. It is the same old political science with new words.

Spadaro rightly titles this compendium, *The Policy Vacuum*. The vacuum remains unchanged as a result of the publication of this book. Perhaps the vacuum will never be filled with the sweet light air of understanding; perhaps it will. My feeling is that the great bulk of what is identified as policy studies is, in truth, political science research that has been "translated" using "trendy" policy science terms and concepts but with little change in focus or approach. Perhaps the discipline, on the individual level, should take Spadaro's advice to heart: "does the political scientist have a real contribution to offer by virtue of being a political scientist in public policy analysis or should the political scientist simply become an economist and have done with it?" (p. 15).

RICHARD L. PATTENAUDE

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Doing Justice: The Choice of Punishments. By Andrew von Hirsch. (New York: Hill and Wang, 1976. Pp. xli + 179. $9.95, cloth; $4.95, paper.)

After a century during which rehabilitation of the offender was viewed as the enlightened form of the criminal sanction, the last five years has seen an amazing reversal of opinion among a broad coalition of penologists, humanists, ex-prisoners, and politicians. Where penal reformers once proclaimed that rehabilitation could not be realized because legislatures would not appropriate the necessary funds for treatment programs and that therapy could not be successful in a correctional environment dominated by the goal of custody, contemporary critics are questioning the very assumptions of the model. Not only is there an accumulating literature pointing to the ineffectiveness of rehabilitative programs on recidivism, but the pervasiveness of the discretion required by the treatment model has been of mounting concern. The dominating influence of rehabilitation is seen by many to have made little impact on the control of crime, while at the same time greatly interfering with the values of due process, increasing the length of incarceration, and widening the boundaries of the criminal justice system.

Doing Justice is the product of a four-year examination of the criminal sanction by the Committee for the Study of Incarceration, a wide-ranging group of scholars chaired by former United States Senator Charles Goodell. Instead of the conventional reliance upon rehabilitation as the preeminent justification for the criminal sanction the committee substitutes the concept of "deserts." As argued in the book, someone who infringes on the rights of others does wrong and deserves blame for such conduct. The sanctioning authority can choose the response that expresses moral disapproval: punishment. Criminals should be sanctioned because they deserve to be punished, not as a treatment and not for the prevention of crime, but rather because it is demanded by justice.

Some may argue that to base a system of criminal justice on this goal is sterile because there is no apparent utility to the punishment, yet, as the book points out, general deterrence is implicit in the sanction since "desert is necessary to explain why that utility may justly be pursued at the offender's expense" (p. 51). The converse is also true since if one seeks to justify punishment as deserved, deterrence is necessary to deal with the countervailing concern with the suffering inflicted.

To operationalize the concept of deserts it is necessary to determine the amount of punishment that should be inflicted and the principles guiding this decision. Unlike rehabilitation which emphasizes that the sanction should fit the needs of the individual offender, deserts focuses on the seriousness of the crime which the offender has committed and the number of prior convictions. Toward this end "seriousness" depends upon the harm done by the act and the degree of the offender's culpability. Punishment should be fair and thus commensurate with the offense.

The idea of commensurate-deserts with its emphasis upon the past and upon the seriousness of the offense undercuts the conventional argument for indeterminate sentencing. The authors sketch a sentencing system of fixed terms whose highest penalty for offenses other than murder is five years and most would be less than three years. In addition, they advocate such alternatives to incarceration as fines, restitution and probation. It is the length of the sentence that will probably be the focus of legislative debate. This issue may be the cause for the dissolution of the coalition supporting reform.

Although not directly addressed, the problem of the wide discretion now given judges, correctional authorities, and parole boards under the aegis of the rehabilitation model undergrids the committee's concern. By shifting the emphasis of the criminal sanction to deserved punishment and fixed sentencing, the authors evidently presume that the need for discretion will vanish. Policy makers should be aware that the organizational needs of the criminal justice system will undoubtedly require that administrators retain some flexibility. In the fixed-sentencing plans proposed in a number of states it would appear that discretion to determine length of sentence has been shifted to correctional officials through generous "good time" provisions.

Deserts appears to be a much sounder justification for the criminal sanction than the much-criticized rehabilitation. Von Hirsch and his colleagues have written a thoughtful book that demands attention. As legislatures respond to the increasing evidence showing that the inflated goals of crime control through offender rehabilitation are not being achieved, the concept of deserts provides an attractive alternative for the doing of justice.

GEORGE F. COLE

University of Connecticut

On Synthesizing Marxism and Christianity. By Dale Vree. (New York: Wiley, 1976. Pp. xvii + 206. $14.95.)

The subject of this book is important both to political scientists and to scholars in disciplines concerned with philosophical and theological reflection on politics. The argument is distinguished by charity, common sense, and intellectual integrity. The author proceeds with serenity and patience to demonstrate that participants in the "Christian-Marxist dialogue" in recent years have only deluded themselves to the extent that they have believed it possible to achieve anything approaching a "synthesis" of Marxism and Christianity, for such a synthesis could be accomplished only at the cost of denying what was essential to both positions. Nonetheless, the author cannot be called a "cold-warrior," for he concludes:

> There is a great deal that can be said about Marxist-Christian dialogue which is affirmative in character. It is, no doubt, desirable that Marxists and Christians should talk to one another rather than kill one another. Moreover, no great harm is done to Christianity if Christians collaborate with Marxists in the building of a better society—so long as the Christians are under no illusions that they are engaged in salvific or redemptive activity, so long as they respect the liberty of other Christians to come to different political conclusions, so long as they do not turn Marxist claims into articles of the Christian faith, and so long as they can be reasonably sure that a government led by Marxists will not persecute Christians or eradicate Christian values.
>
> Marxists and Christians *can* engage in dialogue, they can work together—without engaging in dubious synthetic ventures. But however dubious those ventures are, it must be conceded that they are the products of genuinely good will, not ill will.

By letting the various representatives speak for themselves, Vree unmasks the unchristian elements of so-called "radical Christianity." He links these contemporary advocates of Christianity's adaptation to Marxism with the Gnostic sectarian tradition. Thus, Harvey Cox's division of history into three epochs (tribe, town, and technopolis) is shown to be rooted in the chiliastic sectarian tradition going back at least as far as Joachim of Fiore's three ages of the Father, Son, and Holy Ghost. Juergen Moltmann's brand of Pelagian futurism and self-salvation is discussed in comparison with the central tenets of the Great Church over the centuries. On the Marxist side, Vree points out the disparity between the revisionist interpretation of Marx by Roger Garaudy and the actual writings of even the "young Marx." The assumption that one can be a Marxist while affirming a radically voluntarist concept of freedom and denying the need for Communist party discipline cannot possibly be sustained by even a casual reading of the relevant texts of Marx and Lenin.

Dale Vree, a National Endowment for the Humanities Fellow at the Hoover Institute, has provided us with a valuable account of a significant intellectual trend in our disoriented age. That he himself speaks with clarity from the perspective of what he understands to be "orthodox" Christianity is a particularly refreshing aspect of the volume. His objective clearly is to transcend, not to mirror, the crisis of our time.

It is the author's achievement to have shown that the common meeting ground of the participants in the "dialogue" has been neither "orthodox" Marxism nor Christianity but romantic gnosticism. He has been substantially aided in attaining this insight by his commitment to Catholic Christianity, thereby demonstrating the relevance of that commitment to his political science, without confusing or equating the one with the other.

DANTE GERMINO

University of Virginia

Understanding Rawls: A Reconstruction and Critique of *A Theory of Justice*. By Robert Paul Wolff. (Princeton, N.J.: Princeton University Press, 1977. Pp. 224. $13.50, cloth; $3.95, paper.)

It is perhaps through John Rawls' monumental work, *A Theory of Justice*, that social scientists have had their most significant contact with the recent resurgence of normative political philosophy. Rawls' methodology has been of particular interest to economists, political scientists and social psychologists, since tools of their disciplines and some of their hypotheses play an important role in Rawls' argument. These tools and hypotheses are employed as part of a systematic conception of justice which constitutes an attractive alternative to the all-too-easy dismissal of the moral evaluation of the political order as the mere expression of culturally conditioned attitudes and feelings.

Wolff focuses on the Rawlsian attempt to derive principles of justice from a hypothetical original position, a set of constraints on choice to which the conceptual apparatus of decision theory can be applied. Wolff sees the set of problems arising from the attempt to prove that certain specific principles would be chosen in the original position as central to the evolution of Rawls' thought. Rawls' early article, "Justice as Fairness," is presented as containing the key element of Rawls' conception of justice, namely, the device of a bargaining game under the constraints of the original position. Wolff then argues plausibly that later complications in Rawls' theory are revisions designed to meet critics' objections and produce a clear "solution" to the deliberations in the original position. Central to Wolff's interpretation is his claim that Rawls "never gave up the dream" of deducing principles of justice from a game-theoretical original situation, "using to the end such terms as 'proof,' 'theorem' and 'theory' " (p. 5).

Wolff advances many criticisms of this approach. Along with many other critics, he finds Rawlsian constraints on the original position either obscure or as controversial as the conclusions derived from them and doubts whether the bargaining game has a determinate solution at all. Many of these objections are central, although more than a few will be familiar to those already well acquainted with the (ever-expanding) literature on Rawls' work. Wolff also advances interesting, if hardly fully developed, criticisms of Rawls' conception of social science, and his use of that conception in his theory of justice.

But while Rawls has indeed emphasized the idea of deliberation in an original position, and while it is his application of decision theory to such deliberation that has attracted the attention of many social scientists, there is another theme running through Rawls' work to which Rawls is giving increasing emphasis.

According to this alternate reading, Rawls' principles of justice, with their attractive emphasis on maximizing the well-being of the worst off, are not to be defended by appeal to a game-theoretic bargaining situation. Rather, they are to be defended by their congruence with our conception of persons as free and equal moral beings. Constraints on the original position are to be justified on the moral grounds that they reflect such a conception and allow deliberation by autonomous equals. *A Theory of Justice*, seen in this light, is an attempt to show how a society which conceives of its citizens as free and equal persons is thereby committed to organizing its basic institutions. Indeed, in Rawls' recent essays, particularly "A Kantian Conception of Equality," *Cambridge Review* (February 1975), appeal to a hypothetical bargaining game is virtually absent.

If Rawls' theory of justice is seen in such a light, some of Wolff's criticisms miss the point. For example, he explains Rawls' introduction of the Veil of Ignorance as an attempt to tighten up the bargaining game and criticizes it accordingly. But in his recent paper "from Fairness to Goodness" (*Philosophical Review*, 1975) Rawls justifies the introduction of the Veil on entirely different grounds. There, Rawls declares that "there are, then, at least three different reasons for excluding information from the original position: it would permit self- and group-interest to distort the parties' deliberations; it refers to contingencies and accidents that should not influence the choice of moral principles; or it represents the very moral conceptions . . . that we seek to understand in light of other and more basic notions."

This moral defense of restrictions on the original position, and of the substantive principles which Rawls claims would be derived there, may well have problems of its own. For one thing, it is far from clear that parties to the original position would agree on a single conception of justice, however the constraints on their deliberations are construed. And, as Wolff points out, Rawls' appeal to respect for persons as morally free and equal beings looks suspiciously like an attempt to establish moral truth by appeal to consensus.

Be that as it may, Wolff's emphasis on the game-theoretic aspects of Rawls' work pushes into the background what may be its greatest contribution to our thought. Thus, in an all-too-brief concluding section, Wolff (citing both Marx and Nozick!) dismisses Rawls' theory, and the liberal tradition from which it springs, as altogether too abstract from "the real foundations of any social and economic order" (p. 210). But can Rawls' theory be so easily dismissed, once its roots in the value of respect for persons are made clear? Either Wolff's quasi-Marxist alternative, which is mentioned but scarcely developed, also appeals to such a moral foundation, in which case both Rawls and Wolff are engaged within a common framework, or it does not appeal to respect for persons, in which case it is unclear why anyone should adopt it.

Wolff's book contains much useful exposition and criticism of an important aspect of Rawls' approach to social justice. And even if some of the discussion—particularly Wolff's remarks on the cognitive status of the findings of social science—tends toward the impressionistic, it is often stimulating and provocative as well. But, if it is the morality of respect for persons that makes not only *A Theory of Justice* but the more humane forms of liberalism worthy candidates for our allegiance, Wolff may have missed a substantial point in Rawls, one which may be of greater significance than those upon which he focuses his critical fire.

ROBERT L. SIMON

Hamilton College

American Politics

The Political Socialization of Black Americans: A Critical Evaluation of Research on Efficacy and Trust. By Paul R. Abramson. (New York: Free Press, 1977. Pp. v + 195. $12.95.)

Paul Abramson's book, which addresses the basic question: "Why do black children feel politically less efficacious and trusting than white children do?" represents a restatement, extension and elaboration of his 1972 article published in the *Journal of Politics*. Abramson has published a thorough critical review of the research in this area—the best I have read.

The real contribution of the book is not to be found in the value of the four explanations he examines. The question why black children tend to score lower on the wide variety of scales, indices and items (yes, single items), which purportedly measure their feelings of political efficacy and trust, is not, I am afraid, an intellectually compelling one either in political science or, for that matter, in society. Most of us think we know the answer anyway simply as a result of social and historical awareness, if not empathy or direct personal experience. Discount empathy and experience, if you will, but the findings reviewed in this book do not shed much additional light on this question. The reason: Few studies do more than describe correlations by racial groupings in one washed-out replication after another. As a result, the reader will finish this book with the major question still unanswered.

In any case, having offered these gloomy initial observations about the state of theory and research in this area, let me summarize Abramson's conclusions: The first explanation, that the answer to the question can be found in differences in intelligence, is rejected—happily, I might add, since this is a much too sensitive matter to be addressed with the crude data political scientists have generated. Since Abramson himself recognizes the highly questionable, if not nonexistent, theoretical basis for this explanation, not to mention rickety empirical references, one is led to wonder why this so-called explanation is even examined since the criteria for selection are no better than those for rejection. In short, it's a non-explanation based on conceptually clouded work that confuses nature and nuture, intelligence and achievement, and personal efficacy and political efficacy. Best omitted.

The second explanation, that differences reflect variation in the "political education" of black and white children, is also rejected for lack of evidence. The notion that different political values, e.g., participant vs. subject orientations, are emphasized in schools depending on race lacks any consistent support. Of course, to expect to find such *consistent* support denies what we know about variations in the public schools of this country: Namely, variations associated with regions, urban/rural settings, school districts, school and teachers that many parents believe could have a substantial effect on the content and quality of their children's education (if not their future success, if we are to believe Jencks).

The third and fourth explanations, which suggest that racial differences in political efficacy and trust reflect the different social and political realities confronted by black and white children, taken together can be linked to some supporting evidence. Even here, however, the findings are mixed. Part of the problem is theoretical in that Abramson attempts to treat the explanations separately. Even he seems to recognize, however, that his attempt to distinguish between the two involves arguments that lack conceptual rigor, clarity and awareness of other relevant literature.

For some time history and social science have suggested a clearly observable link between relative deprivation, defined in socioeconomic terms, and political behavior. There is also a rather well-developed literature attesting to the importance of such "bread and butter" issues and electoral results. We also know that in societies, such as this one, where so much of daily life hinges upon government policies, that the relationship between one's well-being and politics is frequently strong and direct whether it be the amount of an AFDC check, a yearly appropriation from the state legislature or the price of heating oil and gasoline. There is even more reason to think that the poor of this country, who live closer to law enforcement than most of us, are even more aware of this link.

Finding himself belly-deep in the perplexing morass of the covarying qualities of these two explanations, Abramson allows that it "may be difficult" to evaluate them separately. He goes on to consider the possibility of combining them (again, presumably unaware that scholars such as Brinton, Cantril, Davies and Gurr have written books about this—not to mention de Tocqueville and Marx) but then rejects the idea because, in his words, "this would probably serve little useful purpose for a higher level explanation would probably not lead us to generate propositions not already advanced by the two lower level explanations" (p. 111). The fact that an attempt is made in this context to separate social deprivation from political reality is not to deal with reality; this distinction reveals more about the atheoretical nature of this work than anything else suggested in the book.

The narrowness of perspective and approach that characterizes much, though not all, of this work requires some comment. It seems that our efforts to define ourselves as a separate discipline has led to a preoccupation, if not obsession, with strictly political concepts. Following the lead of voting behavior research, political socialization work has borrowed adult measures, representing adult concerns, phrased in adult language and, in adult fashion, administered them to children who, as Piaget and Kohlberg have shown, view the world differently. This can only distort our understanding.

The limits of this research are also due in part to our failure to lift our eyes from computer printouts to take into account the broad scope of the political socialization process that would include everything from standards of fairness applied in the family to experiences such as the hostility or generosity of classmates, teachers and parents in recently desegregated schools. What is the relationship between such primal experiences (not the sterility of socioeconomic status) and a child's racial and political identity? We *know* racial awareness develops in pre-schoolers; we know racial judgments are linked to a child's identity; and we know that identity is a prime determinant of *most* behavior—intimate, social and political. Yet Abramson's review of the literature reveals that no studies have attempted to explore such relationships beyond elementary attempts to examine the relationship between socioeconomic status and feelings of personal efficacy or self-confidence.

With this in mind, researchers should direct more attention to clinical, biographical and in-depth studies. There is a wealth of insights concerning the political socialization process in the work of scholars such as Lasswell, Erikson, Lane, Barber and Adorno et al. Robert Connell's *The Child's Construction of Politics* also presents an unusually rich and imaginative study that goes beyond merely correlating "attitudes" that may or may not exist. The sensitive and persuasively written observations of Robert Coles on this subject warrant attention. Coles documents not only that "social deprivation" and "political reality" are prime determinants of a child's feelings of political efficacy and trust, but also, to a considerable degree, *dictate a child's willingness or unwillingness to reveal or share, in questionnaire fashion, such feelings;* a fact of major importance that is, unfortunately, frequently ignored in the studies discussed in this book.

The main and noteworthy contribution of Abramson's book is that he has exposed, intentionally or not, the vapid quality of much of the research he has reviewed. The work tends to be, as he indicates throughout the book, shallow, devoid of theoretical insights, and perhaps misleading. It is clear that a new generation of scholars in this area have followed too long in lock-step fashion the format of earlier research—the same questions, indices and, of course, observations. There is little confidence that the marginal variations in results are not simply an artifact of sampling and data collection procedures, questionable index and scale construction, if not outright statistical error, rather than the real or evoked attitudes of the subjects.

Now that Abramson has identified these problems, perhaps the mold that has produced such a tradition will be broken.

JAMES W. CLARKE

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Campaign Money: Reform and Reality in the States. Edited by Herbert E. Alexander. (New York: Free Press, 1976. Pp. xiv + 337. $10.95, cloth; $2.95, paper.)

Political scientists and other students of the political process have become increasingly interested in the effect of campaign finance reform on political behavior. This book is one of the early examinations of the nature of such reform and what it means. It is made up of ten case studies written by state political journalists, sandwiched between an introductory essay by the editor and a synthesizing epilogue by columnist David Broder.

The objective of this project, funded by the Carnegie Corporation, was to assemble information about campaign finance practices in the states, about recent reforms in state campaign finance laws, and about the effects of these reforms on the state elections in 1974. Each case study discusses relevant state reforms in the immediate post-Watergate atmosphere—disclosure, ethics, limitations, enforcement procedures, and occasionally public financing—within the context of that state's politics. The book touches on the theoretical aspects of election financing, but it is aimed "at providing practical information about how the state systems actually work and how change is (or is not) accomplished" (p. 3).

Perhaps the most significant theme that emerges from the various accounts is that reforms were not very fundamental. Almost every state legislature extended preexisting legislation. They made reporting and disclosure more stringent, set spending limits within a framework close to present expenditures, lowered contribution limits somewhat, closed only the most flagrant violations and loopholes, and made enforcement mechanisms slightly more effective. However, the basic means of financing elections remained the same. Few states considered—let alone adopted—such fundamental changes as public financing, prohibition of large contributions, or public support for parties, and through parties, support for candidates. Contrary to the popular conception, reform was basically a "tightening up" of an earlier system, making it a little fairer and a little more open, eliminating the more flagrant abuses.

Moreover, the meaning attached to a reform component varies considerably. The imposition of a limit on personal contributions in New York means that a candidate can spend no more than $105,000 of his or her family's money in the primary and $250,000 more in the general election. Overall spending in that state is "limited" to $4 million for a statewide race, $40,000 for the state senate, and $25,000 for the assembly. Disclosure in Pennsylvania means the filing of one report 30 days after the election. In Texas, unopposed candidates do not have to file a report. Some states continued to allow multiple committees, circumvention of spending limits, and laundering of money. Many states put more strength in the laws than they did in penalties or enforcement mechanisms. On the other hand, some very strict reforms were enacted, such as California Proposition 9, which imposed very low spending limits (10 percent less for incumbents), instituted a state-distributed ballot pamphlet, imposed strict conflict of interest disclosure and lobby regulation and established a reasonably powerful enforcement body.

The 1974 rhetoric and atmosphere of reform, honesty and condemnation of big money was sometimes absent in practice. Many candidates campaigned on a platform of changing the system to prevent the influence of wealthy individuals, corporations and big unions while they continued to finance their campaigns from these sources. Some candidates decried unsavory practices like laundering of money while this very abuse was going on in their campaign.

Indeed, it was clear that reform neither reduced the high costs of seeking the more visible offices, nor did it reduce the problems of underfinancing contests for less visible offices. For those running for senator, governor or other statewide offices, the limits were sufficiently broad, the costs sufficiently high and the larger contributors sufficiently willing to maintain spending levels. For legislative and local races, however, overall spending levels continued to range considerably, such that great disparities existed between contestants in the same race, interest groups continued to dominate candidates' funds, and party support was lacking, suggesting that business went on largely as usual in many areas.

The increased documentation of the flow of campaign money has opened up the process. It has become much easier to trace the source of a contribution and one can now discern the nature of a candidate's supporting coalition. Documentation has also confirmed many suspicions about how campaign finance operates through multiple committees, loans, fund transfers, in-kind contributions, and imprecise reporting. Disclosure is not a guarantee against impropriety.

A set of questions can be raised about the potential impact of existing and proposed reforms on the political process. How does one explain the differential impact of other variables—the competitiveness of the race, general economic conditions, the nature of the office sought—from the impact of reform? Do the reforms add to the incumbent's advantages, reducing free access to elective office? Will disclosure, prohibitions and limits on contributions and other reforms affect the nature of electoral and governing coalitions? Will a more visible, more regulated system have any impact on public policy? And finally, what will a fundamental change like public financing of candidates do to the already "weakened" condition of American parties? The information base made available by reform will enrich political scientists' opportunity for investigation into these and other questions relating to campaigns, elections and parties.

This book is a useful account of the issues and problems of campaign financing. Using much case material gleaned from the public records, it demonstrates the difficulty of regulating patterned behavior, and particularly the difficulty of asking politicians to regulate themselves. It explains what campaign finance reform did and did not do, illustrating how complex the issue really is.

ROBERT AGRANOFF

Northern Illinois University

Financing Politics: Money, Elections and Political Reform. By Herbert E. Alexander. (Washington, D.C.: Congressional Quarterly, Inc., 1976. Pp. xv + 299. $4.50, paper.)

The spate of federal legislation since 1971 which seeks to reform U.S. campaign financing has attracted the interest and scholarly involvement of social scientists. The number of studies relating to campaign financing by political scientists (and some economists) has been steadily growing in recent years. The amount of information made available by more rigorous disclosure provisions of recent legislation promises to make campaign financing an even more promising area of inquiry in the future. These new disclosure provisions help to produce data which can be used directly in the studies, and increase the possibilities for scholars to use questionnaires or other devices to generate data for such studies.

No one has examined campaign financing longer or more diligently than Herbert E. Alexander of the Citizens Research Foundation. The activities of Alexander and the Foundation now span nearly two decades and the resulting studies have been very important in expanding our knowledge about this fundamental process. In fact, all scholars concerned with campaign financing are indebted to Alexander; because of his efforts, we now have campaign financing data covering several elections.

Therefore it is always important when a new publication by Herbert E. Alexander appears. His latest publication, *Financing Politics,* is also significant because it marks the first publication in the Politics and Public Policy Series which is planned by the Congressional Quarterly Press. *Financing Politics* is similar to other Herbert Alexander publications. In meticulous detail, he provides information about the total cost of campaigning, from whom campaign funds have been raised, how much has been spent for what, what contributions can be traced to various interest groups, gifts of wealthy Americans over several presidential election campaigns, and a brief history of campaign techniques and their relationship to campaign costs. The book relies on data from 1956 onward with special emphasis upon presidential election years, although data from some "off-year" elections as well as developments in some states are also presented. Most of the book, however, was written before the 1976 election, thus making it impossible to provide for that election the detailed fund-raising and spending data provided for other election years.

This book's major contribution to the literature of campaign financing is its presentation of funding and spending information over several election campaigns which provides a historical perspective lacking in discussion of single elections. A second contribution is the detailed discussion of the extensive federal legislation relating to campaign financing which began with the passage of the Federal Election Campaign Act of 1971. The provisions of these various acts are discussed throughout the book and are listed in an appendix. Alexander provides a brief analysis of the impact of these reforms, but leaves to other studies a thorough, systematic examination of the impact of the legislation, which really could not have been made before the 1976 election anyway. The wealth of information provided here will make the book invaluable to political scientists.

Despite the book's contributions, it has several deficiencies. Scholarly literature steadily produces findings relevant to many of the questions raised in this book, yet, we find no attempt to summarize or synthesize this research and its relevance for some of the important questions of campaign financing. In fact, research findings from several Citizens Research Foundation-sponsored studies of state and local elections over the past two decades were not incorporated. Those who would like to see a synthesis of all major research on this subject will be disappointed in this work.

Finally, the book breaks no new ground in answering several important questions about campaign financing, such as the relationships between contributors and candidates, particularly as these delineate the mechanisms through which public policy decisions are influenced; the characteristics, motivations, and policy preferences of contributors, particularly larger contributors, in comparison with voters; the ways in which different methods of solicitation might be utilized to expand contributions, just as we presently study campaigns to discover voter response to techniques and issues; and the decisions about allocation of funds for various campaign techniques by candidates and the relative effectiveness of such spending decisions over several elections. These are not all easy questions to answer. Past studies by Alexander and others have provided important starts toward answering some of them. The historical perspective provided by this book will be helpful as we move on to examine these and other questions.

DELMER D. DUNN

University of Georgia

The Committee of One Million: "China Lobby" Politics, 1953–1971. By Stanley D. Bachrack. (New York: Columbia University Press, 1976. Pp. xi + 371. $14.95.)

The title of this book is a little misleading in that it does not deal with what used to be thought of as the "China lobby" of the late forties and early fifties with which the name of Alfred Kohlberg, the importer, was often associated, and which consisted, according to Ross Y. Koen, of a central core of Kuomintang functionaries, and many individuals and groups sympathetic with the regime of Chiang Kai-shek and hostile to the People's Republic of China (PRC). That was the so-called "China lobby." The work under review is about the Committee for One Million (COM) which was created in 1953 to collect signatures on a petition opposing the admission of Communist China to the United Nations, was reorganized in 1955 as the Committee of One Million with the assistance of Marvin Liebman, an advertising entrepreneur, and went out of existence in 1971 after Liebman had left the organization and American policy towards China had become more congenial. The book is a minute examination of papers and pieces of papers relating to the committee donated by Liebman to Stanford University when he folded up his public relations agency and went to London to become a theatrical producer.

Although there is relatively little political analysis in the book, there *is* a story line developed year by year from the beginnings of COM to its demise in 1971. It begins in 1953 with the Committee for One Million Against the Admission of Communist China to the United Nations, the idea for which is said to have come from a man by the name of Nicholas de Rochefort, who may or may not have been an agent of the CIA (p. 55). The signatures were eventually obtained, representatives of the Committee presented the petition to President Eisenhower, and he told them what everybody knew anyway—namely, that it was not United States policy at the time to support the admission of Communist China to the United Nations. Among the signatories were 53 members of Congress, most of whom later tended to act together on China questions until the election of 1964 devastated their ranks. The committee became the Committee of One Million in 1955; its program was broadened to include other pressure and publicity activities against Communist China besides the admission question; Liebman left the firm that had been handling publicity for COM and established his own advertising agency in 1958, and as executive secretary of COM worked with and through its steering committee of notable public figures as a very diligent promoter of anti-Chinese Communist causes.

Like the American Communist Party in the 1930s, the Committee of One Million prospered when its agitational goals coincided with those of the government and the country, and it ultimately collapsed when they did not. For example, of ten resolutions against Communist China introduced in the 83rd Congress, only three were submitted by Committee activists, indicating that the Committee was not moving policy but flowing with it (p. 92). In July 1956 a resolution opposing the seating of Communist China in the United Nations was adopted by unanimous votes in both houses of Congress. Following suit, the national party conventions that summer both adopted planks opposing the seating of Communist China in the United Nations. COM took credit for the result in the conventions and said that it had occurred because the committee had published two full-page advertisements aimed at the delegates (p. 128). Puffery is an amiable swindle practiced alike by statesmen and politicians but even Chanticleer didn't claim that he could manipulate both Republican and Democratic conventions. Only the sun.

The story of the book is really the story of Liebman. He tried to get tax-exempt status for COM on the theory that it was like two other tax-exempt organizations, the American Association for the United Nations and the Institute of Pacific Relations, but he failed (p. 163). According to the author, Liebman was active in countering the expected publication of Koen's *The China Lobby in American Politics* and in promoting the writing of a book on the "Red China lobby" to answer a work that was never officially published (p. 171). Liebman seems to have been a strenuous activist—forever writing letters, getting material printed in the *New York Times* and the *Congressional Record,* planning petition drives, collecting contributions, making trips, and so on.

But Liebman's activism did not escape rebuke from time to time by members of the committee. Representative Thomas Dodd of Connecticut reacted against Liebman's imprudent attack against a Yale faculty and student group that thought, in 1965, that it was time to reappraise China policy (p. 230). Senator Paul Douglas of Illinois forced Liebman to move COM offices from Liebman Associates, to which they were returned after the senator's resignation from COM in 1966 (p. 251). And in September 1968 Representative Judd and Liebman became estranged when Judd accused the latter of sending out a "Committee-related letter to eighteen newspapers over Judd's signature without obtaining Judd's authorization" (p. 259).

In his last chapter, "An Appraisal," the author lists a number of interesting empirical and moral concerns to which the story of COM is said to be relevant, but which are not pursued very far in the text. The reader gets only Liebman's side because that is what was in the files. In the matter of Judd's complaint, for example, we do not even have the offensive communication to 18 newspapers because it is not in Liebman's files. The development of the Committee for One Million in 1953 is only briefly described because the publicity material for the period, says the author, is not in Liebman's files (p. 80). What is reported, however, is faithfully set out in great detail and would provide useful data for a study of the sociology of organizations or the ecology of administration.

EARL LATHAM

Amherst College

The Transformation of Southern Politics: Social Change and Political Consequence Since 1945. By Jack Bass and Walter DeVries. (New York: Basic Books, 1976. Pp. xi + 515. $15.95.)

If there is a growth industry in American political science, the study of southern politics certainly qualifies. Inevitably each new work is compared with the classic by V. O. Key Jr., *Southern Politics in State and Nation* (1950). Bass and DeVries candidly invite such a comparison. Their book is designed to build directly on Key and his concentration on black people as an explanation of the pattern of southern politics. They follow the outline of Parts I and II of the Key work: an introductory overview, followed by a state-by-state analysis, rounded off with southern relations to national politics. Their work, like Key's, is based on extensive interviews with southern politicians and is supplemented with some voting result analysis and a public opinion survey.

The reader should not expect to find in this work Key's penetrating insights, but there is much interesting political material fitted carefully and logically to an important theme. The authors have a journalistic flair that makes the book interesting reading and they are well grounded in the history of southern political parties.

The authors' theme is stated in their title. Since Key, they argue, the South has been transformed by the entrance of black people into southern politics. In *The Impact of Negro Voting* (1968), William Keech suggested that black political participation was not always followed immediately by substantive policy payoffs. Bass and DeVries take a more optimistic view (except that their view excludes "complacent" North Carolina where both Keech and the authors now live). They argue the black has changed "from object to participant" and the attitudes of white politicians have shifted sharply to meet the new political reality. To indicate the political consequences of this change, they quote from an Andrew Young speech: "It's like the old preacher says: We ain't what we oughta be; we ain't what we're gonna be; but thank God we ain't what we was" (pp. 3, 56). As important as the liberation of the blacks, the authors argue, is the gradual liberation of whites from the twin emotions of guilt and racial hatred.

The first three chapters trace the source of this revolution: the federal court regarding civil rights and reapportionment, the decline of regional differences in America, demographic changes in the South, and the wavering course of southern Republicanism between "the southern strategy" of Goldwater and Nixon and a moderate progressivism. But the authors especially spotlight the role played by blacks willing to take personal risks to gain a measure of political power.

The result, the authors argue, is a resurgence of the Democratic party in the moderate or liberal image of such leaders as Jimmy Carter, Reuben Askew, Dale Bumpers, Andrew Young and Barbara Jordan. The coalition is one of old-style populists, New Deal liberals, labor, and blacks.

The state chapters discuss the peculiarities of the government, politics and prominent personalities of each individual state. Each begins with an overview of what has happened since Key wrote, followed by sections that usually touch on the activities of blacks, Republicans and Labor and describe briefly the factional alignments that developed around the major political figures of the period. Relegated to an appendix are the familiar county maps of vote results used so effectively in Key's work, along with accompanying graphs, so that when the reader reaches them, they are like a dinner of cold cuts. One has to go back and review the detail to fit in their message.

The chapter on southern representatives and the nation discusses the division between old-style and new-style southern Democrats and the South's dwindling influence on congressional committees. Key's roll-call analysis is repeated to demonstrate that even on civil rights issues, the southern representatives no longer present a monolithic front. The authors conclude that "The South retains some distinctive regional qualities, but it has joined the nation's political mainstream" (p. 407).

This book is suited as a textbook supplement in an undergraduate course (assuming it will be published in paperback). It is well written, full of anecdotes, and leaves the reader with a feeling of hope about democracy in general and the American polity in particular. Jimmy Carter is not given a more extensive coverage than he deserves as a new-style southern governor, but he and a number of other figures now prominent on the national scene, are there and stimulate the reader's interest.

The book's potential appeal for undergraduates or for a popular audience may make it less appealing to the political scientist. Our local newspaper review of the book complains that "the authors occasionally get bogged down in voter precinct totals and other important political scientist details." The political scientist is likely to complain of the reverse. This is one of a growing number of studies by political scientists and historians that turns away from the computer outputs on welfare expenditures among the states, to concentrate on the specific political battles taking place in individual states. Like Key, it assumes these battles have significance on the way citizens live their lives. Of course the danger of such studies from the political scientists' view is that they could "get bogged down completely in political anecdote and local color."

Bass and DeVries can not really be accused of this. They do have a theme, which is that a state's factional politics is significant; i.e., that black political participation has had important social, economic and political redistributory consequences. But the political scientist reader interested in state politics is likely to hunger for more, an analysis of party factionalism in a period of change that ties into a theory. Few writers on state government and politics since Key have been so self-consciously analytical as he was. As my colleague Joseph Schlesinger points out, Key had the job of explaining how a static system worked; "Ole Gene" Talmadge, Huey Long, the Virginia Museumpiece and the Florida rat-race all were part of a common pattern. Bass and DeVries have the more difficult task of explaining a system in change. Their description of the ingredients for change is satisfying and their argument that significant and enduring consequences have resulted is at least a plausible hypothesis. But the political "how it works" remains hazy. It is perhaps unfair to expect more of this work, written to appeal to a mass audience and also to be a standard source for political scientists. They have also come closer to Key's standards than many studies of state politics have done.

CHARLES PRESS

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Revenue Sharing: Methodological Approaches and Problems. Edited by David A. Caputo and Richard L. Cole. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xvi + 183. $18.00.)

Events surrounding the recent reenactment of General Revenue Sharing (GRS) have provided an important opportunity to examine the usefulness of contemporary social science methodologies in meeting the information needs of policy makers. Two years before reenactment, the Ford Foundation, the National Science Foundation's Research Applied to National Needs Program (RANN), and a number of other groups funded research efforts to determine: (1) How funds were distributed to recipient jurisdictions by the GRS formula; (2) What the expenditure patterns of revenue sharing were in municipalities; (3) What effects the program had on local policy planning and citizen participation processes.

These questions, as well as the adequacy of the methods used to answer them, are discussed in Caputo and Cole's *Revenue Sharing: Methodological Approaches and Problems.* The aim of the book, according to the authors, is "to provide as diverse and representative an array of approaches, positions, and conclusions as possible regarding GRS" and, in addition, to raise "a series of important methodological and substantive questions concerning the impact and utility of policy analysis" (p. x).

The book consists of six summaries of research attempts to evaluate GRS and three essays evaluating the research. Only one of the evaluation pieces, an article by Donald Rosenthal, actually cites specific GRS research to any extent. However, it does so almost entirely in footnotes without providing examples of deficiencies identified. Consequently, charges of "narrowness" (p. 153) and "political naivete" (p. 154) leveled at the body of research, which Rosenthal calls "peculiarly uninteresting" (p. 157), are by-and-large so unspecified that a reader cannot judge for himself. As a researcher in the field, I feel it would have been preferable if Rosenthal had cited specific studies in his criticism instead of referring to "most formula research" (p. 154), "much research in expenditures" (p. 156), and "citizen survey data" (p. 157), especially since he lists specific studies in footnotes.

The two editors' closing essay compares overall research findings in a number of studies and finds much consistency in them. The chapter also raises questions about the failure of the research to address such broad issues as the relation of GRS to other New Federalist reforms and the need for meaningful political organization at the local level. Although these questions are provocative, the remainder of the book fails to prove that the failure of most GRS research to address them is as serious as Caputo and Cole claim. Researchers, especially those trying to meet policy maker's deadlines, need not answer every question.

More seriously, Caputo and Cole avoid some significant issues which they should have explored. An interesting piece by Trudi Miller Lucas of NSF, in the first section of the volume, points out that the National Science Foundation purposely funded research based on a number of perspectives. She argues that while social science research is theoretically objective, the methods employed by social science embody assumptions that have value connotations–e.g., assumptions about what is important. By purposely soliciting research based on a number of different points of view "objectivity in a research program may be maximized by . . . covering all major partisan positions in the program's aggregation of study" (p. 115). Caputo and Cole comment only obliquely on the validity of this approach. After discussing comparability problems caused by "laissez-faire" research, they make the point that diverse approaches allow decision makers to choose studies which support "just about any position they wish to take" (p. 169). This should not concern researchers, they claim, since decision makers typically do not use policy research "to help them arrive at their position" (p. 169). Their article ends by criticizing narrow research which is not able to offer "a comprehensive plan to better the overall quality of life for citizens" (p. 171), although Caputo and Cole do not say who would utilize such plans.

Like the evaluation pieces, the other articles in the book are of mixed quality. Richard A. Smith and David Cozad analyze the Actual Use reports submitted by communities participating in the program in terms of a conventional list of political, social and ecological characteristics. They find few generalizations possible. As Catherine Lovell points out in her more sophisticated article which follows, most of the data collected through Actual Use reports are "likely incorrect and in some cases absurd" because revenue dollars are fungible (p. 55).

An article by Alvin D. Sokolow summarizes the impact of GRS in five counties but provides no description of the methods used to gather findings. An analytical piece by Paul Dommel suggests a conceptual approach to GRS evaluation based on economic theory and omits reference to any of the data gathered by researchers over the past few years. Finally, an article authored by Edie N. Goldenberg, James H. Foesch, and Thomas J. Anton, summarizes a number of significant findings about the fiscal, program, and citizen participation impacts of GRS and raises an important methodological question. It points out that many of the questions treated by the GRS research were no longer relevant when the renewal debate reached its peak. Most GRS research focused on the uses of funds and political effects attributable to the program. The civil rights issues and fiscal relief issues so important in the actual GRS renewal debate arose *after* most researchers had embarked on their research. Goldenberg et al. point out that: "academic techniques such as survey research are inherently inflexible; once questions have been formulated, research is too "locked in" to particular problem formulations and data needs to adapt to changes in the problems being studied or to recognize real world conditions that violate analytically convenient assumptions" (p. 103).

Had the Caputo-Cole book dealt in a more systematic way with the important questions raised by Lovell, Lucas, Goldenberg, and others, it would have been much more useful to both social scientists and policy makers. The present volume is not a how-to book for policy researchers or a comprehensive analysis of research conducted. It belongs instead to the genre of anthologies compiled by academics who have to put something out every year or two.

STEVEN A. WALDHORN

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Cities, Suburbs and States: Governing and Financing Urban America. By William G. Colman. (New York: Free Press, 1975. Pp. x + 350. $12.95.)

Colman's stated purpose is to "provide an overview of the principal issues involved in governing and financing urban areas and more particularly, to try to fill a gap in the current popular and academic literature" (p. 1). The gap is in how fiscal and legal structures of state and local government affect the delivery of urban services. Because there has been a large amount of recent theoretical and empirical work in this area, a summary and synthesis by someone with Colman's experience would certainly be valuable.

The style chosen for the work is primarily descriptive. Lots of information is provided, especially on recent developments in the courts, inner-city schools and manpower and health services. There is considerable data on city-suburban trends, but the focus on city versus suburb does not illustrate that suburbs are homogeneous residential areas, but are themselves extremely diverse. By far the weakest section concerns fiscal relationships where the lack of treatment of recent rethinking on property taxation, empirical work on the suburban-urban exploitation hypothesis, and fiscal implications of low productivity increases in service-intensive activities are not considered. Scholars, lawyers and judges in states with strong home-rule constitutions will also be surprised to learn that no matter what the state constitution says, the state government can do anything it wants to local governments.

The descriptive format lends itself to presenting a variety of perspectives on most issues and there is no push for a particular urban government reform in spite of occasional use of old reform-through-consolidation rhetoric. Along with the diverse perspectives, however, the work would have been strengthened by some attempt to indicate some of the critical differences in reasoning or evidence used by advocates of different policy preferences instead of just summarizing their conclusions. Colman also often presents strong policy conclusions which are supported neither by logic nor empirical evidence, but statements of this kind stand out from the rest of the work so they are easily recognizable.

The most puzzling aspect of the entire work is its stated intention to treat legal frameworks, and institutional structures related to urban service delivery, followed by the total lack of reference to the bulk of recent systematic work in this area. Even popularized summary studies such as those published by Resources for the Future and the American Enterprise Institute are neglected—let along the Sage Urban Affairs Annual Review volumes devoted to these issues, studies published in academic journals, or better studies of institutional reform such as the *Task Force on Local Government Reform Report* in California. It is simply not enough to rely on newspaper and magazine articles, government reports and a few monographs from Washington-based public interest groups to keep up with analysis of urban institutions and service delivery.

As a consequence of both style and source materials, the book is very uneven. There is much good material, but some parts are inadequate or were already seriously dated prior to publication. It is almost totally lacking in serious and rigorous institutional analysis, and it is certainly not a summary or synthesis of the state of knowledge in urban institutional and fiscal analysis.

ROBERT L. BISH

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The State of the Presidency. By Thomas E. Cronin. (Boston: Little, Brown, 1975. Pp. 335. $6.95, paper.)

A central theme of Thomas Cronin's book is that exaggerated and unrealistic expectations of the modern presidency have become increasingly disproportionate to its effectiveness. Presidents have often responded to such expectations by distorting their priorities and neglecting the effective implementation of policy, while exaggerated expectations and disappointing results cause public disaffection. This situation, convincingly described in the chapter entitled "The Cult of the Presidency," underlies many troubling dilemmas about the modern presidency.

Throughout this book (some of which has been published before) Cronin probes the dilemmas involved in making the presidency effective and accountable. The book derives from extensive interviews conducted with more than 100 former White House staffers, cabinet members, and other leading officials in both the Johnson and Nixon administrations. The account is enlivened by extensive use of verbatim comments from his respondents. The work synthesizes the author's research and participant observation in a lively and interesting style.

Many previous treatments of the presidency have stressed the president's role in policy making. Cronin discusses the reasons for policies whose results have been lacking and the causes of presidential neglect and incapacity to implement policy. In several chapters, entitled "The Braking of the President," "Palace Guard Government," "The Cabinet," and "Making the President an Effective Executive," the author suggests many reasons for the gap between policy and results. Among these is the growth of the White House staff and executive office of the president, which have become a sub-government insulating the president and contributing to the decline of the cabinet and the executive departments.

Accordingly, several chapters are devoted to the growth of the White House staff and the Executive Office of the President (well-known as the article called "The Swelling of the Presidency") and the pattern of White House/ Cabinet relations. In a short time the White House staff and Executive Office of the President have become an operating bureaucracy and a headquarters for coalition building. No recent work traces so well the growth in the White House staff and the development of a presidency thereby. In large part, this expansion is the result of the shortcomings of the cabinet and departmental bureaucracies. Cronin discusses the tensions between the White House staff and some cabinet secretaries and their departmental administrations.

Like Neustadt and others, Cronin stresses the many constraints on the presidency, especially in domestic policy making, arising from the fragmented bureaucracy, the weakness of the cabinet, the parochialism and lagging responsiveness of Congress. Foreign policy receives less attention. In his chapter entitled "A Job Description of the Presidency," he argues that the job has become so complex and over-demanding as to elude description. The demands of the position have become excessive, outdistancing the capacity of institutions to hold it to account, thus leaving vacuums where authority may be abused.

In the final chapter the author deals with the problem of accountability. "The modern Presidency . . . may be unaccountable because it is too strong and independent in certain areas and too weak and dependent in others" (p. 290). Presidents have been escaping accountability by justifying policies in terms of national security, which protects them from public scrutiny. The increasing use of executive immunity, executive orders, executive agreements and emergency powers keeps presidential conduct from proper accountability. Hence, the problem of making "the Presidency safe from Presidents and making the Presidency safe for democracy" (p. 292). Cronin evaluates current methods for securing accountability, as well as such recent proposals for referenda, extending the term of office to six years, a plural executive, a "no confidence" vote and several other ideas.

Cronin pays some attention to the failure of Congress to check the presidency. Congress has willingly acquiesced in the growth of presidential influence. But to ask Congress to reform would require it to assert greater independence and autonomy than it has yet exhibited. Enlarging congressional staff, an often suggested reform, would not enhance the countervailing influence of Congress. Nevertheless, Cronin argues, Congress should increase its oversight over the White House staff and Executive Office and bolster the departments against the incursions of the White House staff. In the end, Cronin would like to see presidents adhere to party platforms and to their political parties.

Cronin's analysis and synthesis is welcome in a literature which dwells too much on particular presidents and thereby contributes to waves of cultish adoration or revisionist denigration, neither of which adds to our understanding of the presidency. However, one may be disappointed that although Cronin offers coherent and rich chapters, he has no unifying theory. The presidency, he argues, cannot be approached by selecting one dimension. But, a question must be asked that goes beyond Cronin's excellent work—why has the subject of the presidency received so little theoretical treatment? Except for a few works, discussions of the presidency have been deficient in attempts at theory building and empirical research. The paradigms so widely used in other fields of political science have scarcely touched examinations of the presidency. Can it be that the subject lacks theoretical analogues or that prevailing empirical methods are hard to apply, or both? Even if both explanations were true, they alone do not provide an adequate answer.

An ideological slant must be held to blame. We have been wedded for too long to the ideology of the Progressives which holds that the presidency is the people's tribune against the special interests. This judgment has blocked the detachment political scientists have brought to other subjects. Research on the presidency has not liberated observers from ideological dispute. Yet, opposing views often stem from *identical* empirical descriptions of the presidency.

Cronin's account helps to remedy the scholarly neglect of the developmental continuities and discontinuities of the presidency. Underlying each presidential administration, however unique, are many continuous developments; the policy coalitions that support and oppose presidents cut across several administrations. Such coalitions also link presidential recruitment and presidential governing. Neustadt gave us a penetrating exchange model of the presidency, showing why presidents must bargain and persuade. Cronin adds to this model by pointing out new resources and vulnerabilities, learned from the rise and fall of Johnson and Nixon, and thus leads us away from our innocence about the presidency. The development and variability of presidential influence still challenges explanation. Cronin's insightful work contributes significantly in that direction.

LESTER G. SELIGMAN

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Congress Against Itself. By Roger H. Davidson and Walter J. Oleszek. (Bloomington: Indiana University Press, 1977. Pp. xiii + 306. $15.00.)

Congress Against Itself is a case study of the attempt, during the 93rd Congress, to reform the House committee structure. The authors describe the effort from the creation in 1973 of the House Select Committee on Committees—popularly known as the Bolling Committee—through the emasculation of the committee's plan on the House floor in late 1974.

Both authors served on the Select Committee staff and thus had an excellent vantage point for observation. That they made good use of this advantage is evident throughout; the narrative varies from very good to excellent. The section on the committee mark-up sessions is especially fascinating. The members of the Select Committee were all dedicated to committee reform and all had strong ideas on the course such reform should take. They were also aware that, if their plan were to have any chance of adoption, strongly held principles would have to be tempered with political expediency. Because of the authors' staff positions, they are able to describe how each member weighed these considerations and how the committee as a whole arrived at a position which all members could support and which they believed would be acceptable to a House majority. The description of the floor debate is also excellent and, like the section on the mark-up, gives the reader the feeling of actually being there.

The author's purpose, as articulated in the preface, extends beyond the description and analysis of this particular reform effort. They are interested in the conditions under which "pressures for institutional change build up," in the "factors [which] facilitate or hinder the adoption of institutional changes" and in "what . . . the struggle over committee reform tell[s] us about long-term prospects for maintaining the vitality of Congress" (p. xi). In dealing with these complex issues, the authors are not completely successful. The chapter "The House as an Organization: Institutional Decentralization" is a nice brief analytic history of the House providing a perspective on the problems with which the proposed committee reforms attempted to deal. In their analysis of the factors influencing the adoption or rejection of institutional reform, the authors are confronted with the inevitable problem which arises when one attempts to generalize from a case study. They explain convincingly why the reforms failed to be adopted; they do not really grapple with the question of the circumstances under which basic reforms in committee jurisdictions might be adopted. The authors discuss various tactical and strategic errors made by the committee but it is hard to believe these made the difference between success and failure. The Select Committee's plan—and perhaps any major jurisdictional change—simply ran counter to the self-interest of too many Congress members.

The House's track record during the early 1970s shows that, under certain circumstances and in certain areas, the House can make major internal changes. One cannot conclude that the House is incapable of self reform. A somewhat more careful comparison of the changes the House has made with those proposed by the Bolling Committee in terms of their implications for power redistribution might have been fruitful.

In sum, while the authors do not answer all the hard questions they raise, *Congress Against Itself* is an excellent case study of the Bolling Committee's reform effort and well worth reading.

BARBARA SINCLAIR

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The Unseen Revolution: How Pension Fund Socialism Came to America. By Peter F. Drucker. (New York: Harper and Row, 1976. Pp. 214. $8.95.)

The "unseen revolution" was actually an unnoticed technical innovation introduced precisely in April of 1950, precisely by Charles Wilson, the chief executive officer of General Motors. The innovation was a new pension fund for GM workers, and Peter Drucker's book is an elaborate and significant commemoration of that event.

Employee pension funds go back a century. Drucker himself reports that there were 2,000 pension funds in operation by 1950. The GM proposal was novel, and worthy of commemoration, because it was purely capitalistic—company-initiated, company-financed, and placed as an "investment trust" in the hands of professional asset managers responsible to the company, with no union role whatever. The investment trust was not to be in government bonds or other fixed-interest-bearing obligations—as was the case with the pension program of the Bell System, our largest. Nor was the GM program to be comprised of the stocks of the company for which the employee worked—as Sears had established in 1916 and as was being popularized in slightly different form by Louis Kelso not long after the GM program was established. Nor was the GM program to be an "industry-wide" fund such as TIAA-CREF. The GM pension fund was, and is, a corporate-financed, corporate-managed program based upon the rules that Wilson had set forth at the beginning: "professional independent management of corporate pension funds as "investment funds"; minimal or no investment in the company for which the employee works; no investment in any company in excess of 5 per cent of the company's total capital; and no investment in any company of more than 10 per cent or so of the total assets of the pension fund. . ." (p. 10).

Initial union reaction was opposition and suspicion. In fact, a UAW officer proposed that the union lodge an unfair labor practices complaint against GM, since the pension proposal could have no purpose except to undermine the union (p. 6). But eventually the union came to see that the advantages far outweighed the disadvantages. The plan was adopted, and very soon it spread so far beyond General Motors that it truly revolutionized retirement finance in the United States. By 1975, there were roughly 50,000 pension plans in the United States, and nearly all of them were of the GM variety. Analysis of the investments of these pension funds and the investments of closely related pension funds such as TIAA-CREF, reveals that at the end of 1974 the pension fund portfolio had nearly $150 billion worth of shares in publicly owned companies. That comprised about 30 percent of the stock market value of all listed companies—and the trend is upward. The pension fund is supplanting social security as the primary source of retirement income. In Drucker's well-argued view, the social security system is fast becoming a welfare system of relevance mainly to those aging persons who cannot build up enough retirement income from their low-paid or infrequent work.

Drucker's identification and assessment of the pension fund phenomenon is a major contribution to the literature of political science as well as economics; and it should be studied with care, especially by the growing number of policy-oriented political scientists. Even his speculations on the future of American politics are worthy of careful study by political scientists. The demographic shift toward the older age cohort, which is producing a near-majority of retirees and those who can clearly see retirement, is beginning to express itself in politically relevant interests. The pension fund is a concrete manifestation of this, and there is no particular expectation of a change in this development until the demographic pattern changes—and this may not come to pass until beyond the end of this century.

But what kind of a revolution is this? Is the pension fund revolution socialism? Drucker ardently believes this to be the case. He subtitled his book accordingly. He encases in quotes every concept and every "ism" in the book—except "pension fund socialism." Up to a point it is extremely difficult to deny him his argument. The capital market is now increasingly the pension funds themselves? The market is no longer Wall Street, which is a depressed industry being overtaken by "structural obsolescence." Worker and capitalist have become one and the same; the "capital fund" is created out of "labor income"—whether payments are made by employee, employer or both—and is channeled back through labor income. Yet, while this is perfectly compatible with Marxism and at odds with classical and Keynesian economics, "pension fund socialism" is vastly superior to Marxism because it leaves capital and labor mobile and free. The fact that it is totally private (except that these funds are now insured by the federal government against companies going bankrupt) does not, to Drucker, alter the socialist nature of pension fund socialism. Pension funds split the property atom once over again beyond the split identified by Berle and Means in the 1920s. This new property is not merely in a collectivist form called stock; it is contingent rather than certain. It does not persist beyond the life of the recipient and his or her spouse. That is in itself a kind of socialism (cf. especially pp. 34, 43, 71, and 148–49).

But beyond a few substantial points, Drucker has not really identified a socialism, although it is a useful word for gaining attention. What he has identified is gerontocracy. He painted the entire picture of a gerontocracy and then didn't perceive it. It brings to mind a story about Thomas Edison, when he burst into his wife's bedroom in the middle of the night, displaying his new invention, the lightbulb. As the story goes, he then held it up to his ear and said "Hello, hello!" My own position is more like Mrs. Edison's, who, according to the story, turned away soberly and said, "O.K., but will you please turn out the lights and come to bed."

Drucker's book is the best vision yet of the "post-industrial society": a gerontocracy based upon insurance and re-insurance. The role of government is very prominent in both aspects of this, to varying degrees. In the United States, thanks to private insurance companies and to the Charles Wilsons, a large and growing proportion of insurance is provided privately. The government role is growing but not at as fast a pace as the private aspect of insurance, if Drucker is correct. But re-insurance (including indemnification of pension plans, insurance of bank deposits, provision of loan guarantees, etc.) is very governmental in the United States, as everywhere. In all of this, the only thing that has really been socialized is *risk*. Pension funds and the other forms of insurance and re-insurance are the administrative expression of the socialization of risk. It is a vast and public hedging not so much against present failure as future decay and dependency.

When you put all of this together there is not a nickel's worth of classic socialism in a carload of insurance. As Drucker himself admits (e.g., p. 112), capital formation continues to come heavily from retained earnings; pension funds will join in as second (and perhaps first later on) owner of capital, yet by law they cannot control corporations, and by a preference they go along with corporate board decisions; and as our population grows older its interests will tend toward the most conservative of corporate decisions.

What we have, then, is not a new form of socialism at all. Instead, we have a *state of permanent receivership.*

THEODORE J. LOWI

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Congress: Keystone of the Washington Establishment. By Morris P. Fiorina. (New Haven and London: Yale University Press, 1977. Pp. xi + 105. $8.50, cloth; $2.95, paper.)

Morris Fiorina has written a fascinating book growing out of a short commentary he published in the *American Political Science Review* (March 1977) on the factors which keep members of Congress in office. Drawing upon the work of Erikson, Mayhew, Ferejohn and others who have noted the ever-growing number of House districts which are no longer marginal, Fiorina seeks to explain why this is the case. The book appears to be aimed largely at an undergraduate audience; its style is easygoing, it is well written, and it is both short and inexpensive. Yet, it is also a tract that no student of Congress can afford to neglect.

The basic thesis is that there *is* a Washington establishment, consisting largely of members of Congress and the bureaucrats with whom they interact. This establishment help keeps incumbent members in office by facilitating casework and other constituency service functions that virtually ensure their reelection. The "case of the vanishing marginals" is thus solved by pinning the "blame" on the bureaucracy, but only as a co-conspirator with the original plotters, the incumbent members themselves. Members win votes not by their stands on issues, not by simple gerrymandering, and to a decreasing degree on the basis of party identification support. Fiorina does not deny that these factors exist in congressional elections, but they do not, in the aggregate, account for the decline in competition in congressional districts over the last two decades. Aggregate survey research data from the biennial SRC/CPS election studies give little support for the alternative thesis that increased incumbent security can be accounted for by voter recognition of either candidates in general or incumbents in particular (relative to their challengers). Such a thesis would be quite consistent with the constituency service argument suggested by Fiorina. What is one to conclude, then?

Fiorina's argument is that the electoral effects of constituency service on the congressional vote are (borrowing Fiorina's pun) at the margin. The entire voting population does not need to know of the member's work on behalf of his or her constituents, but only enough to account for the incumbency advantage (calculated by Erikson as about five percent). Fiorina visited two similar districts in the same state, one still marginal and the other formerly so. He spoke to people who see the members in their constituency service positions and concludes that, indeed, there is support for his thesis among the "informed" constituents themselves. The analysis of the two districts (chapter 4) is not only a useful teaching device, but also does indeed serve as a mini-laboratory in the search for explanations of the vanishing marginals (and why 72 of the 74 freshman Democrats seeking reelection in 1976 were successful). Unfortunately, the Bunsen burner in the laboratory is flickering. The choice of the districts (which are not revealed, but can be deduced if one is persistent enough; I admit to some outside help) seemed more appropriate just a short time ago than it does upon publication of the book. District A did fluctuate considerably between 1952 and 1972, after which the incumbent retires, but the seat was becoming much safer in recent years. In 1974, the GOP successor had a tough race; the comments of district observers that 1976 would be different were well borne out by the election results: he received over 70 percent of the vote. District B tells a different story. A six-term Democrat, supposedly a "vanishing marginal," vanished himself—losing to a Republican challenger despite all the advantages on his side. Fiorina's book should have included these 1976 results for they generally support his thesis. (Some references to this election are in the footnotes.) Even without them, however, the argument does indeed appear to be sound in general. It explains how many new liberal members of the 94th Congress "bought their freedom" from policy disagreements with constituents through casework and were reelected.

The remainder of the book is largely an elaboration of these themes, but with a good section on the normative implications of this Washington establishment. Fiorina clearly is among the pessimists, but there appears to be inadequate attention to the policy-making role that members can take on, given this freedom (I am among the optimists). While the Washington establishment is *not* malevolent (p. 3, emphasis in original), it does not seem to offer much hope for democratic government. Students and scholars alike should read this book along with Schattschneider's *Semisovereign People* to get alternative views as to what democracy *should* be like. And they should read it with Davidson and Oleszek, *Congress Against Itself,* for a more detailed picture of the linkages between bureaucrats and members of Congress—which Fiorina only briefly touches. But they should read it, above all, to understand that our government can be either only as good as, or much better than the people who elected it.

ERIC M. USLANER

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Toward a Planned Society: From Roosevelt to Nixon. By Otis L. Graham, Jr. (New York: Oxford University Press, 1976. Pp. xvi + 357. $11.95, cloth; $3.95, paper.)

Otis L. Graham, Jr., an historian at the University of California, Santa Barbara, has written a very useful book on U.S. economic-social planning (and quasi-planning efforts) covering the period from FDR through Nixon, with a few thoughts on Ford. The book is a combination of institutional history—covering such institutions as the National Resources Planning Board, the Domestic Council, Council of Economic Advisers, etc.—and an argument for stronger social planning. It is more successful at the first task.

The author's assumptions are significant, and are best expressed in his quotation from the introduction: "Planning assumes that modern industrial society requires public intervention to achieve national goals; assumes that such intervention must touch all fundamental social developments; must be goal-oriented, and effectively coordinated at the center; must be anticipatory rather than characterized by ad hoc solutions and timing dictated by crisis" (pp. xii–xiii). Given this set of criteria, it is not surprising that the author's normative evaluation of the history he reports reveals disappointment and frustration. However, whether one agrees with his personal stance or not, the descriptive history is very well done (from the viewpoint of a political scientist interested in administrative developments) and quite valuable in the conceptual perspective it provides on how we have gotten to where we are and how the very partial, episodic planning we have done so far may relate to Planning (with a capital P) in the future.

Some of the particularly interesting sections of Graham's book include: an argument that inadvertent policies have had greater impact than those planned ones in the area of New Deal resettlement programs (p. 36); the argument that partial planning and "broker interventionism" made Planning harder; seeing housing and urban renewal planning requirements of the past 20 years as steps toward national growth policy (p. 162); the argument that federal grants have produced more planners, but not more planning (p. 164); the ups and downs of land-use planning, 1969–73 (pp. 219–32); and the anomaly of conservative planning (pp. 258–63).

Toward a Planned Society presents the author's ideal, and a history of some steps along the way. For all of those interested in national indicative planning or state-local land-use planning, it provides important background.

MICHAEL D. REAGAN

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The American Catholic: A Social Portrait. By Andrew M. Greeley. (New York: Basic Books, 1977. Pp. vii + 280. $15.00.)

In The American Catholic: A Social Portrait, Andrew Greeley marshals all the arguments he can against what he labels the elite stereotype of American Catholics as an anti-intellectual, racist, morally rigid and conservative mass.

Instead, Greeley envisions the American Catholic both as an argument against "the melting pot" and as a counter to the claims of superiority emanating from the intellectual elite who are, both by definition and design (according to Greeley), anti-Catholic. Greeley's counter-claim is that Catholics are both different and better.

Greeley demands that readers deal with his work on two levels—first, as an ethnic Catholic polemic, á la Michael Novak, Richard Gambino, and Gino Barone, directed to a broad, nonscientific audience. In this aspect, the work merely supplements the other authors cited, and, although Greeley is facile with words (a gift he would attribute to his Irish Catholic heritage), writers like Novak have already made the case better and more succinctly.

The second level of the work is more important to social scientists, particularly since Greeley claims the main audience of *The American Catholic* is academic. He asserts that *The American Catholic* effectively shatters some of the "myths" about Catholics. Unfortunately, although Greeley's argument is morally acceptable the presentation of it is flawed—and flawed in such a way that the entire book risks not being believed by those whom Greeley most wishes to convince.

In the first place, many of the "myths" he cites are not, and have not been, fostered by reputable social scientists. Nearly all of the election studies, as well as the public opinion literature accept the generalization that Catholics tend to be more Democratic, and more "liberal" in attitudes than white Protestants, particularly when social class is used as a control. Although the popular literature may have the image of an ethnic Catholic as the prototypical hard-hat (anti-black, anti-radical), previous investigations (and even Greeley's earlier work) do not support this claim. The intellectual gains of the American Catholic are made clear in the Carnegie Commission Study of higher education. And it has recently become evident that, at least in terms of income, there is no "religion gap." The myths that Greeley wants to dispel are myths that emerge from the early fifties when Rogin demonstrated Catholic support for Joseph McCarthy in Wisconsin and Lipset found Catholic attachment to the "loyalty oath" (although Greeley links those "myths" with the Galbraiths, Duttons, and Burnses of the Democratic Party).

A second major difficulty with Greeley's work is the logic on which it is based. The myths are wrong, according to Greeley, yet the Catholic is still discriminated against. It makes no difference that the income level of several Catholic ethnic groups exceed that of WASP America, nor that certain ethnic groups have a higher proportion of members enrolled in graduate school than the national norm. Catholics, according to Greeley, remain outside the bastions of power in Ivy League and corporate America. On the one hand, Catholics are successful; on the other, they are oppressed. This division allows Greeley the freedom to make either argument whenever one suits his purpose more than the other.

The third, and most critical difficulty with Greeley's work is the use he makes of available data. None of the data are new; they have been previously collected and analyzed by others. Greeley is eclectic in presenting the data: instead of providing straight percentage tables he uses some percentages, some deviations from the mean, some betas, some rank ordering, some Z scores, and inconsistent controlling for income and residential location. Occasionally, Greeley ignores data entirely—after describing the "Catholic social ethic" he admits that there are no data available to test its existence, blames elite funding agencies for their unwillingness to provide support, and offers a limited sample of city-dwelling women, which indicates that Catholics were slightly less likely to be planning to move from a neighborhood than Protestants. Here Greeley offers no controls, simply a percentage table, with religious differences of from 5 to 15 percent on attitudinal questions.

On p. 118, Greeley presents Z scores on racial questions for various ethnic groups. The Z scores, computed after the effect of education and no "avoidance factor" have been taken into account, tell us that Slavic Catholics, whom the elite malign as racist, are much more likely to vote for a black mayor than other white groups. What Greeley passes over is the fact that the data come from the Campbell-Schuman 15 City Study of Racial Attitudes, and that the Slavic Catholics in the sample are primarily from Cleveland. By 1968, Cleveland already *had* elected a black mayor, and Carl Stokes was then in the honeymoon period of his first term. Slavic Catholics were not necessarily demonstrating a lack of prejudice, but may have been simply reacting to a political fact of life. Additionally, the small Ns on which many of the conclusions are based are troublesome.

Finally, the book is sloppily edited. Tables are occasionally unclear. Correlations are presented as integers (p. 121), books are mistitled in footnotes (p. 111), tables and text do not match (p. 180), and aberrant question marks appear in tables (p. 193). The style varies between political sloganeering and social science jargon. It's too bad, since Greeley would like to make the case for a somewhat ignored and occasionally abused American group.

KATHLEEN A. FRANKOVIC

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A Government of Strangers: Executive Politics in Washington. By Hugh Heclo. (Washington, D.C.: The Brookings Institution, 1977. Pp. xvi + 272. $10.95, cloth; $4.50, paper.)

When Leonard D. White wrote the initial volume in his "systematic study of American ideas about public administration," it seemed perfectly natural to him to inquire into "George Washington as an Administrator." He drew the conclusion that

> [President Washington] possessed a deep-seated understanding of the importance of good administration both as a means of consolidating popular support for the general government, and as an essential source of strength of the government itself. He understood good administration to be characterized by integrity, system, energy, reliance on facts, relative freedom from detail, and due responsibility to Congress. (*The Federalists,* 1948, p. 114).

In these two sentences White offers his estimate of how Washington viewed the age-old relationship between politics and administration, between making policy and making policy work.

Hugh Heclo addresses this same time-honored quest in an important book with a most revealing title—*A Government of Strangers.* We all know that politics and administration have become staggeringly complex since George Washington first set about making a government. In this book, Heclo displays a modern dimension of this intricate connection as it is evidenced in the associations of appointed executives and career bureaucrats. Few topics would seem to merit greater attention in the study of American government and politics and yet the only major work that even comes close to treating this subject is now 20 years old (Marver Bernstein, *The Job of the Federal Executive*). Thus, Heclo's book will justifiably find its way into the basic library of all serious scholars of national policy development and administration.

In preparing this work, Heclo interviewed approximately 200 political and career executives. Drawing on his notes from these encounters, he seeks to set the problem, describe the setting, identify the people involved (i.e., the appointed executives and career bureaucrats), specify the nature of their associations, and suggest reforms. Since he writes so beautifully, permit me to use his words in tempting you to order this book or move it to the top of your reading stack if you already have it. I offer the following selected quotations, ordered to the outline he follows (see above).

The Problem: "Political leadership is transient . . . [and] problematic . . . heavily contingent on how political executives themselves choose to act. . . . By comparison, bureaucratic power is non-problematic and enduring. . . . The essence of *bureaucratic power* lies in both the service and resistance that officialdom can offer to political leaders" (p. 7).

The Setting: ". . . political leadership has become more bureaucratized; the incentives have also grown for politicizing the bureaucracy. . . . Trying to figure out who is who is not a passing stage for the uninitiated but a preoccupying and enduring question in Washington politics" (p. 35).

The Political Executives: "The conventional image of Washington in-and-outers erroneously suggests a political team of utility players, when what actually exists is a one-time sequence of pinch hitters" (p. 103). "[T]he real challenge to the political executives' statecraft in Washington . . . [is] to move in two worlds—the tight ingrown village life of the bureaucratic community and the open, disjointed world of political strangers" (p. 112).

The Bureaucrats: "However they may feel about their political superiors, higher civil servants usually believe they can do better by keeping their relationships going rather than by trying to win every little battle" (p. 146). "Gradualism, indirection, political caution, and a concern to maintain relationships—these suggest some of the common dispositions in higher career officials' behavior that go beyond conventional images of bureaucratic inertia" (p. 148).

The Associations: "The real test of a political executive's statecraft is his ability to institute the changes he wants without losing the bureaucratic services he requires. It is a question of needing positive services and not just passive obedience from top bureaucrats. . ." (p. 181). "Political executives can usually do better by evoking conditional cooperation rather than by invoking their authority" (p. 220).

It goes without saying that these few references do not begin to capture the richness of description and analysis in this book. But you can begin to see what Heclo has laid before us, i.e., a set of bureaucratic communities uncertainly led and hardly managed by "here today and gone tomorrow" political executives. Being "one-time pinch hitters," the leadership is not around long enough to learn the tricks of a difficult trade. But tenure alone does not result in wise and resourceful exercise of authority either, for Heclo rightly concludes that the challenge is great, the conditions unsupportive, if not downright hostile. This set of circumstances poses a quite different set of concerns in regard to the growth of government. Less should we be distressed by the emergence of a monolithic and tyrannical administration and more by the improbability of realizing effective leadership of an ever-expanding bureaucracy. In a recent column, Art Buchwald described his encounter with a number of little green people from outer space. "Take us to your leader," one demanded. "We don't really have a leader," was the answer. Hugh Heclo now provides this further response: "But we have a government—a government of strangers."

It should be said that Heclo is not at all satisfied with this state of affairs. In his final chapter he discusses reform. Exercising effective leadership requires "political support, coordination, sensitivity, endurance, commitment" but Heclo is anything but confident that these requisities can be met. Too many forces work against realizing change through major reorganization or urging people to behave differently. "What reforms can do is to try to change the strategic setting so as to create somewhat different incentives and calculations for using people" (p. 249). To this end Heclo proposes "a third force" in the form of a "Federal Service" of "high-level officials . . . more changeable and mobile than bureaucrats but more institutional and enduring than political appointees" (p. 249). This new service would intermediate between the two levels or groups.

Frankly, I am not much taken with this proposal. I doubt that it will overcome the essential problems Heclo identifies. My own tendency would be to think about a political solution—e.g., means by which presidents are held more accountable for who *doesn't* do what within an administration. To return to my opening comments, however, perhaps one thunderous conclusion from all of this is that it is no longer natural, or even in order, to inquire into the "president as an administrator," President Carter's *efforts* to become one to the contrary notwithstanding. Possibly there is no politics; there is no administration; there is only gradualism, indirection, caution, maintenance, continuance, aimless expansion. But you don't need to hear from me on this whole problem. What you need to do is to read Heclo's book and be equally stimulated to reflect on the important political and institutional implications of what he has described and analyzed.

CHARLES O. JONES

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Tweed's New York: Another Look. By Leo Hershkowitz. (Garden City, N.Y.: Anchor Press, Doubleday, 1977. Pp. xx + 409. $12.50.)

The Tweed Ring, according to the conventional interpretation, was simply a band of thieves that dominated New York City politics in the late 1860s and early 1870s and took advantage of its enormous power to loot the municipal treasury. The ring was overthrown when a group of public-spirited citizens exposed its frauds and made sure that Tweed and his fellow criminals were punished for their misdeeds. In this book, Leo Hershkowitz undertakes to revise this interpretation.

Hershkowitz's revisionism, however, does not consist of asking new questions about the place of the Tweed Ring in New York history or about Tweed's significance in the development of American urban politics. Rather, Hershkowitz takes each of the conventional judgments about Tweed and turns it on its head. For example, he attacks the conventional analysis of the role the Tweed Ring played in New York politics in the 1860s not by arguing, as does Seymour Mandelbaum, that Tweed and his associates centralized power in the only way possible in the mid-nineteenth-century city, but by simply denying that a Tweed Ring existed and that Tweed himself was an especially powerful political figure. Rather than asking why the coalition of political forces which supported Tweed tolerated the ring's graft and corruption, Hershkowitz simply denies that Tweed ever stole a penny. Rather than analyzing which elements of the New York business and political communities found it in their interest to engineer Tweed's downfall, Hershkowitz simply dismisses the reformers as nativist bigots and chronic city-haters. The picture that Hershkowitz draws of Tweed, then, is that of an honest and not unusually powerful public servant, who was singled out for vilification by self-styled reformers because he was a friend of the immigrants and had made many important contributions to New York City.

This picture can be questioned on a number of grounds, the first and most obvious being that it is flatly contradicted by Tweed's own words in testimony before investigating committees of the state legislature and the Board of Aldermen. Hershkowitz dismisses these confessions as the desperate efforts of a broken and dying man to secure release from prison, and he harps upon Tweed's failure to provide documentary evidence to corroborate his testimony. But surely one would not expect a politician to keep records of the bribes he received and distributed. Nor is it clear how Tweed's interests would have been served by telling a patently false story of political rings and legislative bribery, which, if constructed out of whole cloth, as Hershkowitz maintains, would not have been credible to his contemporaries.

In the second place, Hershkowitz's claim that Tweed was neither corrupt nor powerful leaves one unable to understand how Tweed, who started life as a chairmaker, accumulated a fortune in excess of $2 million. Hershkowitz anticipates this objection to his argument and seeks to deal with it in two ways. He suggests at one point that Tweed earned almost $1 million in legal fees by serving as counsel for Jay Gould and Jim Fiske in their struggle with Cornelius Vanderbilt for control of the Erie railroad. Tweed, however, had no legal training to speak of, and if one denies that he distributed bribes for Gould and Fiske and used his political influence to help them, it is difficult to see how his services could have been worth $1 million to them. At another point Hershkowitz tries to explain away the fact that the balance in one of Tweed's bank accounts increased in a fixed ratio to the balance in the accounts some minor officials who admitted particpating in the frauds, by suggesting that these individuals, in an effort to implicate Tweed in their crimes, deposited close to $1 million in Tweed's account without his knowledge! But surely the more plausible hypothesis is that this money was deposited with Tweed's knowledge and at his direction, especially since everyone concerned, eventually including Tweed, testified that this is how the money got there.

Third and most importantly, in arguing that Tweed was not an especially powerful politician Hershkowitz utterly fails to grasp the significance of Tweed's role in New York politics. He glosses over the fact that for close to a decade Tweed simultaneously occupied a number of important public and party offices in New York: county supervisor, state senator, deputy commissioner of the Streets Department (later commissioner of the Public Works Department), chairman of the General Committee of Tammany Hall, and Grand Sachem of the Tammany Society. To be sure, the Tammany which Tweed led was not the well-organized and highly centralized political machine it was to become under his successors, John Kelly and Richard Croker. But by simultaneously occupying important positions in the city, county, and state government, in the legislative and executive branches, and in the Democratic party organization and the Tammany Society, and by organizing rinks and combinations of officials within and across these disparate institutions, Tweed personally provided a measure of central coordination within a political system that previously had been extremely fragmented and highly unstable. Hershkowitz fails to tell his readers any of this; though he is an historian, his analysis is strangely ahistorical. Hershkowitz certainly is correct in arguing that Tweed was not the all-powerful political boss his enemies claimed he was, but by failing to situate Tweed within the stream of political change in New York, he underestimates Tweed's contemporary importance, and his importance as a transitional figure in New York Politics.

Although Hershkowitz's central thesis is open to serious question, this is not to say that there is nothing to be learned from his book. He shows in some detail how the reformers were able to exert influence over the judicial system and to twist legal procedures in their campaign to discredit Tweed. This book, then, is less interesting for what it has to say about Tweed than for the light it sheds upon his opponents, and is less valuable to political scientists as a study of the transformation of urban party politics after the Civil War than as a study of the interaction between the legal and political systems in the United States in the late nineteenth century.

MARTIN SHEFTER

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Bias in the News: Network Television News Coverage of the 1972 Election Campaign. By C. Richard Hofstetter. (Columbus: Ohio State University Press, 1976. Pp. xv + 213. $13.75.)

In the late 1960s and early 1970s, Spiro Agnew and Edith Efron (among others) passionately argued that network television news was feeding its viewers a very unbalanced diet of politics and ideas. Their thesis was as simple as their methodology—left-liberal opinions dominate the nation's airwaves.

Using the networks' coverage of the 1972 presidential election campaign as the basis for his analysis, Richard Hofstetter presents a rigorous and exhaustive examination of the Agnew-Efron thesis. His judicious, tentative conclusion is that no persuasive case can be made that political bias significantly affected the content of the networks' coverage of the 1972 presidential campaign.

Hofstetter supports this conclusion with data on the networks' treatment of the candidates, issues, and parties. He devotes one chapter to the networks' 1972 coverage of each of these topics. In every instance the data consistently and strongly show that the overwhelming majority of stories broadcast by the networks cannot be reliably classified as favorable or unfavorable, supportive or unsupportive of any candidate, ideology, or party. The bulk of network news content is simply neutral or ambiguous. Here and there on this or that issue and in this or that situation, the networks may present coverage marginally favorable to a party, candidate, or ideology. For instance, Hofstetter reports that in 1972 the networks devoted more stories and time to McGovern and his campaign (no doubt because he campaigned more extensively), but the stories broadcast about Nixon were more favorable and supportive. Thus, when all the data are examined, no *pattern* of political bias emerges.

Without question the data reported in this book are the culmination of the most thorough, systematic, and reliable content analysis of network television news that has ever been undertaken. Hofstetter and his staff viewed and analyzed every weekday evening network news program between July 10, 1972 and November 6, 1972, and his book and the accompanying working papers (published separately), which explain his methods and procedures, constitute a thoughtful critique of the problems of content analysis as well as an up-to-date manual on how to do it. As a result of the author's concern for methodology and objectivity, however, this is a book that is likely to impress only the scholar seriously interested in the bias question. Yet, because of its methodological soundness and even-handed and critical presentation, the book is sure to be an authoritative source.

As the author is quick to point out, his conclusion of no partisan bias does not support a view that no bias at all is operating in network campaign coverage. What Hofstetter calls structural and situational biases are always present. In the first instance, television selects stories on the basis of how well suited they are to the medium's special brand of communication and how well they meet the demands imposed by the networks' structure and organization as commercial enterprises. Thus, stories that are brief, that have conflict, that use pictures, and that appeal to a large national audience are more frequently displayed. In the second instance, the networks will broadcast stories that reflect the political situation of a particular election—for instance, incumbent presidents who do not usually feel it necessary to wage an extensive campaign for reelection will always receive less coverage than relatively unknown challengers who feel they need all the exposure they can get.

No doubt Hofstetter's book will not resolve the debate about bias in network news nor will its methodology and data be convincing to everyone. As he is aware, the meaning of bias is not easily quantified and one person's bias is the other's objectivity. Nevertheless, this book is a solid first step.

ROBERT D. MCCLURE

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The Courts and Social Policy. By Donald L. Horowitz. (Washington, D.C.: The Brookings Institution, 1977. Pp. 309. $11.95, cloth; $4.95, paper.)

The Jurocracy: Government Lawyers, Agency Programs and Judicial Decisions. By Donald L. Horowitz. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. xi + 145. $14.00.)

The uses—and overuses—of legality is a current topic of widespread concern. A recent cover story in *Newsweek* asked, rhetorically, "Is There Too Much Law?" Writing in *The Public Interest*, Nathan Glazer attached the abuses of an "imperial" judiciary. And in *The New York Times*, Russell Baker explored our nation's most recent affliction, "terminal jurisprudence."

These two books by Donald Horowitz make a major contribution to the debate and deserve widespread attention by other scholars and policy makers alike. However, criticism of the role and power of the courts is not new. What distinguishes these books is that they are not merely a backwash of right-wing ideology, but sophisticated inquiries into the limits of the judicial role by a new-generation scholar. *The Courts and Social Policy* explores two fundamental questions: what *can* courts do, and what *should* they do? What are the limits of their capacity, and what are the boundaries which define their legitimate functions?

Horowitz argues that the unquestioned recent expansion of judicial activity is not simply usurpation of power. It is more accurately seen as the product of converging social forces, including the statutory growth of the welfare state and enormous and increasing pressures by litigants seeking judicial intervention in what formerly were regarded as purely administrative or political concerns. No doubt the judiciary itself has made its contribution through enlargement and extension of key constitutional rights and in expanding the right of access of citizens to seek relief in the courts. But for whatever reason, it is apparent that judges—especially but not exclusively federal judges—are deciding a range of issues far beyond the orthodox boundaries of judicial power.

Horowitz argues that to maintain their effectiveness and legitimacy, judges must recognize some limitations of their role. Courts are not well suited to perceiving the need for social change, and even less well suited to fashioning the rules and remedies required to bring about change. To act as social and political policy makers courts often must transcend the boundaries between judicial and legislative functions. They become less "distinctive" institutions then they were (or were commonly believed to be). Horowitz fears that courts which further blur their image by functioning as hybrids between the worlds of law and politics run a serious risk.

Of the many attributes which undermine the courts' policy-making effectiveness, several are of particular importance. First, as Horowitz notes, judges are generalists. By training and by function they rarely have the opportunity to specialize in a particular subject. They rely on the parties to a case to educate them about the facts and issues. Of course, judges can, and do, read widely and explore beyond the limits of the courtroom for information. But as Martin Shapiro has noted (in Polsby and Greenstein, eds., *Handbook of Political Science* [Reading, Mass.: Addison-Wesley, 1975], pp. 321–72), the more that judges seek elsewhere for the answers to difficult policy questions, the greater the strain on their primary role as impartial arbiters of conflict.

Second, there are severe limits on judicial policy making imposed by the adversary process. Courts are reactive institutions. A "case" framed by a particular set of litigants, may or may not be an accurate reflection of what is in fact social reality. Horowitz shows quite dramatically how even the best-intentioned and well-informed judges can completely fail to perceive the social and political reality on which a good decision must be grounded.

A judge's perception of the facts is often skewed by the need to focus on the "rights and duties" of the respective parties. This is necessary to invoke the jurisdiction of a court, but it often obstructs realistic analysis of the costs and benefits of a particular decision, and limits the consideration of alternatives. Consideration of unanticipated consequences is rarely possible in the judicial setting, at least not openly possible. An institutional form of tunnel vision makes it difficult for courts to address the broader issues which a case may imply but not directly raise.

Finally, even if courts have accurately diagnosed a problem, they are still quite limited in the remedies which they may select; and they are without effective capacity to review the implementation and outcomes of their decisions. Incremental and timely policy adjustments which often make the difference between effective and ineffective policy making are rarely possible in the judicial setting. Fast-moving events may quickly render obsolete the logic and applicability of a judicial decision. Social and political life is not suspended to meet the courthouse pace. Courts are rarely in an optimum position "to measure the relationship between action to be taken and results to be achieved" (p. 107). There *are* occasions when a court may retain jurisdiction of a case and undertake a more active supervisory role. But this is obviously impractical in most cases and where it occurs, most recently on a large scale in school desegregation cases, the judge is situated in an unaccustomed and uncomfortable role in the political spotlight.

Horowitz pursues these themes in four case studies. One focuses on judicial intervention in enforcing the Model Cities Act. Another shows the frustration and futility of judicial efforts to deal with the problem of equitable financing and allocation of resources in the public schools in the District of Columbia. If these two case studies illustrate relatively atypical judicial fare, two others fall squarely into a more conventional judicial mold. *In Re Gault* (1967), imposed new due process requirements on the juvenile courts, and constituted a subject familiar to most judges. *Mapp* v. *Ohio* (1961), mandated the exclusionary rule in search and seizure cases on the state courts and can truly be said to have been the beginning of the criminal justice "revolution" by Warren Court. Yet in neither case did the Supreme Court accurately gauge the probable response to its decision or take account of possible second order consequences.

Horowitz observes that even within the judicial orbit, Supreme Court efforts at reform must contend with an uncertain and unpredictable environment. In all these cases, judges had difficulty in obtaining and dealing with behavioral evidence. Horowitz stresses that there are important differences between social facts and legal facts—hardly an innovative thought—but as he shows very effectively, the distinction may play a crucial role in deciding cases of social policy.

There is no easy solution to this dilemma. A policy of dogmatic judicial self-restraint is no answer, if only (but not only) because the alternatives are no more promising. Horowitz resists the easy assumption that legislative bodies or bureaucracies are necessarily more competent than courts in planning and controlling policy outcomes. The differences are subtle, and for some the choice between them is best made on normative grounds of applied democratic theory rather than on the basis of demonstrated institutional capability.

Horowitz's message, therefore, is one of prudential skepticism. We should understand as best we can the limits of judicial resources, and we should not make demands on the courts which erode their capacity to fulfill a distinctive judicial function.

This is not an immediately satisfying answer. It offers little guidance as to how these dangerous shoals can be avoided. Moreover it provides no satisfactory response to the obvious counter: "So what if courts are imperfect policy makers? There often are no better alternatives to litigants whose grievances cannot be handled effectively in the larger political arena. The rapid rise of social policy litigation may bring the courts closer to the people by being more responsive to their needs, and in so doing they may be forging the tools to play a new and important political role."

The *Jurocracy* is a different kind of book, but it also reflects Horowitz's concern with litigation and the proper role of the courts in shaping public policy. His focus is on government lawyers—"jurocrats"—and on the provision of legal services to the executive departments by the Civil Division of the Department of Justice. The study is based largely on the author's own service in the Justice Department from 1969 to 1971, but is supplemented by additional interviews and subsequented collected data.

This is a narrowly based study, but a rich one nevertheless, reflecting both the insider's experience and understanding and the scholar's theoretical perspective. If the book is primarily about the policy implications of providing legal services to the government, it is also a fascinating excursion into some aspects of bureaucratic policy making marked by the author's knowledge and application of organization theory.

The Civil Division serves as litigation counsel for the executive departments (most administrative agencies handle their own litigation below the Supreme Court level). But all executive departments retain substantial in-house legal staffs. If the Division lawyers are specialists in litigation, especially concerned with protecting the legal needs of the government across a broad spectrum of cases and issues, the in-house counsel are more concerned with the specific programs and practices of their own departments. The clash of interests in this setting is obvious. Yet this division of legal authority reflects an historic decision not to locate all of the government's legal functions either in—or out of—the Department of Justice.

Horowitz shows how this bifurcated organization of legal services affects the advocacy of the government's business in the courts. There are differences of perspective, of expertise, and of training which produce conflict and tension in what is officially a cooperative venture. Division lawyers tend to be younger, trained in the more prestigious law schools, and more likely to regard their service as an apprenticeship opportunity to develop and sharpen legal skills. Department lawyers are older, come from the less prestigious law schools, and have a stronger career commitment to their respective departments. Division lawyers are more apt to serve relatively brief terms, on the way to more lucrative and prestigious legal employ. They are interested in the law in its broadest sweep. Department lawyers, on the other hand, more likely to find career satisfaction within the department or agency, are more attuned to the agency's immediate needs than to the legal needs of the government as a whole.

If this divorce between litigation and legal counseling is of long standing, it is also true that it has become less and less comfortable. The dramatic rise of litigation against the government, and the new willingness of the federal courts to entertain substantive challenges against government programs, make it difficult to justify this continued division of authority. Litigation is no longer merely an ancillary problem which every agency must deal with. It has become a central part of the policy-making process.

The norms of judicial deference to substantive bureaucratic decisions, "apt for a time of national quiescence and deference to expertise, have in large measure given way before a new spirit of skepticism and disdain for excessive specialization" (p. 17). The need for maintaining some correspondence between the law in the courts and the law practiced in the departments has, at times, become acute. Yet inertia and the rigidity of vested interests within the government makes reform difficult to achieve. While recognizing the legitimacy of the interests represented by the Department of Justice in this struggle, Horowitz favors reallocation of some additional litigating authority to the departments, observing that the government has easily survived a dispersal of such authority to the administrative agencies. But he recognizes that there is no single optimal solution, no set of interests or values so distinct and clearly defined that support can be effectively mobilized behind it.

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Protecting Consumer Interests: Private Initiative and Public Response. Edited by Robert N. Katz. (Cambridge, Mass.: Ballinger, 1976. Pp. xxiii + 277. $17.50.)

This volume is an edited version of the proceedings of a three-day symposium on consumerism sponsored by the National Affiliation of Concerned Business Students (NACBS) and held in October, 1974. A cooperative effort between business executives, university faculty, and graduate students, NACBS has produced two companion volumes, on environmental management and corporate social policy, both gleaned from previous symposia.

It was refreshing to read a series of good-quality essays resulting from a conference. Too often, collections of remarks and papers tend to be anecdotal and episodic commentaries rather than solid analyses held together by a common theme. The editor, Robert Katz, who is also managing editor of *California Management Review*, obviously took pains to ensure quality and continuity. It is always desirable to have such conference proceedings available from a commercial publisher, since the results of many symposia receive only a limited distribution or none at all. Particularly in the areas of business and society, consumerism, and economic education, there are few good anthologies and only a handful of journals. Some of the organizations ostensibly devoted to education in these areas do very little to promote interdisciplinary research into the politics of consumer interests, but NACBS has done a considerable service by sponsoring *Protecting Consumer Interests*.

The contributors are for the most part familiar names in the field of business and society, and their affiliations include political science departments, business schools, corporations, law, and the media. Given these varied perspectives, it is not surprising that one dominant theme of the essays is that there is no single conception of consumer "interest." In the essay by David Vogel and Mark Nadel, four case studies exemplify instances where either consumer groups or business adversaries were divided on an issue. Competing consumer groups may take policy positions different from one another, and certain types of consumer action or relief used by one group may not be appropriate in a given case. This idea is conveyed in part 5 of *Protecting Consumer Interests*, "Some Special Industry Cases." None of the contributors, however, seems to challenge the very idea of a single consumer interest, as the "public" interest challenge in the study of public administration, or the "national" interest, outrightly rejected in some quarters of international political studies. Rather, the consumer interests are usually represented by activist groups, public-interest lawyers, or a classic producer-consumer market relationship. That these interests are not always the same introduces an additional notion that the consumer interest is represented by government agencies.

An entire section of the book is devoted to these government regulatory agencies, particularly the Federal Trade Commission and Consumer Product Safety Commission. The FTC is criticized for its excessive attention to the settlement of individual claims rather than to its law revision responsibilities, and for housing pro-business high-level appointments. However, the latter problem may not be directly the FTC's fault. Further, in many areas of consumer legislation, but especially in the financial realm, FTC acts as an umbrella agency, leaving enforcement to others. The Equal Credit Opportunity Act, for example, names 12 separate enforcement agencies in addition to the FTC. The concurrent problems of inter-agency enforcement standards are not addressed satisfactorily in this section, although it ends on an optimistic note with two pieces on standards for industry self-regulation.

Another theme which runs through *Protecting Consumer Interests* is the controversy over consumer redress. For the past decade, the courts seemed to be the only effective political institution for relief from unfair practices or faulty products. Yet some of the contributors now believe that other institutions and groups are providing a forum for consumers beyond redress for past indiscretions. Consumer information groups, ombudsmen, the media, and corporate consumer offices are among these. The selections by Kenneth Phillips and Francis Pollock provide an interesting look at legal services centers and consumer interests in the press, respectively. A short summary of these alternatives to consumer protection are listed in chapter 6, "Emerging Issues on Consumerism," by S. Prakash Sethi. Curiously, the roles of trade and professional associations, and labor unions, are almost entirely omitted. Although these institutions have long histories of political activism and education, they are not mentioned directly, even in Mayer and Nicosia's article on consumer information (chapter 4).

In sum, the major problems with *Protecting Consumer Interests* seem to be omissions rather than questionable interpretations. Perhaps NACBS will reserve space in its subsequent volumes for these issues. For those interested in the study of consumer interests, this book is a welcome addition because of its serious tone and quality analysis. Though short on empirical contributions, it is superior to attempts which consider the issues from a single perspective, often laying claim to *the* consumer interest.

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The New Presidential Elite: Men and Women in National Politics. By Jeane Kirkpatrick with Warren E. Miller, Elizabeth Douvan, William Crotty, Teresa Levitin, and Maureen Fiedler. (New York: Russell Sage and the Twentieth Century Fund, 1976. Pp. xix + 605. $20.00.)

Using as her principal evidence interview and questionnaire data from Republican and Democratic delegates to their respective 1972 presidential nominating conventions, Jeane Kirkpatrick argues that 1972 marked the entrance of a "new breed" into the presidential elite. For Kirkpatrick a presidential elite is "all persons with influence on the selection and presentation of candidates and issues in a presidential election year" (p. 21). She characterizes the *source* of the "new breed" as the growing percentage of "symbol specialists" in the society (college-educated professionals): their *significance* as shapers of new moral codes; their *political beliefs* as weakly supportive of parties and ideologically holistic, intense, extreme, and moralistic; their *participation opportunities* as enhanced by the increasingly permeable party organizations (especially Democratic); the *consequences of their participation* as weakening the ability of the party system to represent voter preferences.

Unfortunately, the study was not planned to evaluate the above model but, rather, to "gain new understanding of the political characteristics of women in American politics" (p. xvii). The shift in emphasis, the author explains, was due to the failure of theoretically expected sex differences to appear in the data and, secondly, her discovery of the "New Breed." What remains of the results of the original research design occupies the final 118 pages of text and constitutes the best statistical analysis to date of women in politics. Still, the main thrust of her argument concerns the model of the presidential elite, and I shall limit my comments to it. To fit her model to the data at her disposal, the author had to make two assumptions which her study was not designed to test; first, that convention delegates are part of a presidential elite and, second, that the 1972 conventions (especially the Democratic) marked a permanent change in elite structure. The author argues the validity of the first assumption mostly by assertion. She does not confront the counterargument that conventions have lost their primary decision-making function, and thus delegates now gather at the convention site as candidate organization loyalists to ratify what has been decided in the pre-convention period. Moreover, recent reforms democratizing the delegate selection process have strengthened the hand of candidate organizations in binding delegates formally and informally to their candidate. The single most relevant delegate characteristic may now be candidate affiliation rather than occupation, education, race, sex, or ideology. Even if future conventions are contested, it is likely that candidate organizations will be the most powerful bargaining agents.

Yet the author writes as if party were the only organizational form whose support was worth measuring. Though there is an extended and useful treatment of role orientation towards party, there is no analogous treatment of candidate organization. In fact, Kirkpatrick's index of *party* organizational support is simply called the "index of organizational support" (p. 136). Furthermore, her narrow organizational focus leads her to misidentify (at least in one part of the book) the reform rules as radicalizing the Democratic presidential elite. She only reluctantly acknowledges what her data clearly show; namely, that ideological orientation was more determined by candidate affiliation than by sex, race, age, occupation, experience, etc. In fact, the combination of quotas and increased candidate organizational recruitment of delegates dampened the correlation between the quota variables and political attitudes.

The author is on firmer ground in her analysis of delegate attitudes and beliefs, especially in her discussion of party organizational support and ideology. She renames, and partly redefines "professionalism" as "organizational support"—the disposition to choose policies and nominees to promote organizational survival. And quite rightly, I think, she defines organizational support as independent of issue orientation. (Thus it is empirically possible to classify a delegate high on support and purist in issue style.) In past research, professionalism has often been conceptualized with "organizational support" at one end of the continuum and "issue purism" at the other. In addition, Kirkpatrick develops theoretically distinct aspects of ideological orientation—extremeness, intensity, holism, and moralism—and provides measures of the first three. (Her prose suggests moralism as an aspect but no measure is provided.) Her conclusions on ideology are clear; for both Republicans and Democrats aspects of ideological style was strong; those high in organizational support tend to have nonideological styles. For Republicans, however, the relationship between organizational support and ideological style was much weaker, suggesting—perhaps—fundamental asymmetries in the party system not captured in the "New Breed" hypothesis. Yet Kirkpatrick's data, as interesting as they are, do not bear on what I have called her first premise—that convention delegates are members of a presidential elite. The emergence of powerful candidate organizations may have eliminated the convention's decision-making role and made delegate attitudes more reflective than causal factors in the convention process.

The second major premise in Kirkpatrick's argument is that the "New Breed" was more than a transitory phenomenon. Her too-ready acceptance of this premise leads her to describe McGovernite "symbol specialists" as a "New Breed" when, in fact, they may well represent the short-term dominance of a particular *kind* of candidate organization.

She does try, however, to support the second premise in a number of ways, none of them completely successful. First, she tells us that the composition and attitude structure of convention delegates has been changing slowly for some time in the direction represented by the "New Breed." She fails, however, to produce any supporting data. Her second argument showing the change in composition of delegates from the 1948–52 period to 1972 is supported by her data but suffers from another problem. It is not methodologically sound to pick what may have been an extreme case—the 1972 Democratic convention—and compare its composition with an arbitrarily chosen baseline (1948–52). The observed change may be a consequence of the particular events of either period. The author tries to escape this difficulty by claiming that the more traditional 1972 Republican convention (which renominated an incumbent and thus was less subject to short-term forces) still showed a percentage gain in symbol specialists from 1948–1972. Unfortunately, the author presents no data on this point, and, even if she had, her own data show 1972 Republican symbol specialists to be nothing like their Democratic counterparts concentrated among the McGovernites.

Finally, Kirkpatrick argues that the increased permeability of party organization (internal democracy, the spread of primaries, the rise in the percentage of Independents) has facilitated the participation of middle-class groups (especially "symbol specialists" and those with low party loyalty and extreme ideological style). If organizational permeability is to result in a specifiable "New Breed," the middle-class groups advantaged by a more open or permeable party system must be homogeneous in ways that conform to Kirkpatrick's definition of a "New Breed." If true, the short-term forces will always produce activists with New Breed characteristics, and the author's 1972 description will have turned out to be a discovery. If, on the other hand, the middle-class groups advantaged by increased opportunities for participation are more heterogeneous, then the short-term forces (events particular to an electoral period) will increase the variability in delegate composition and attitude from one convention to another. Each convention will, so to speak, have its own "New Breed."

The doubtful validity of the author's two main premises leads me to take a more modest view of the book's achievements than is suggested by the title, "The New Presidential Elite." As a report on the 1972 convention, however, the book is a solid contribution to our knowledge of presidential nominating conventions and their role in American party life.

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Change and Continuity in American Politics: The Social Bases of Political Parties. By David Knoke. (Baltimore and London: The Johns Hopkins University Press, 1976. Pp. xv + 192. $11.50.)

Basing his work on an analysis of the University of Michigan's Survey Research Center/Center for Political Studies presidential election surveys from 1952 through 1972 and including the 1956, 1958, 1960 panel survey, the author examines the shifting coalition support for the Democratic and Republican parties from the standpoint of both party identification and voting behavior, focusing on the former. Analyzed in chapters 1–4 are various facets of coalition support (the "social bases")—religion, more refined than usually presented, with a breakdown of Protestant denominations, and church attendance; region, including migration effects; race; residence; subjective (class) and objective (occupation and income) socioeconomic status—and party identification itself. While a causal model of party identification is presented in chapter 5, the remainder of the book (chapters 6–8) focuses on the much-analyzed themes of party switching, independent growth, ticket-splitting, and the decay of the present party system.

Early on the author points out that "this book was written on the assumption that readers know or wish to learn the fundamentals of multivariate data analysis" and that "the manuscript does not presuppose an *extensive* familiarity with the (analytic) techniques. . ." (p. 17, emphasis added). Some familiarity is more than useful though Knoke aids readers without the knowledge of regression, path analysis (for the party identification causal model), and log-linear models with a technical appendix, "Methods of Data Analysis" (pp. 163–73). Since the book is written in a simplified citation style and lacks explanatory footnotes, the appendix might be especially useful for some readers.

A careful reader might disagree with Knoke's unconventional collapsing of the seven-category party identification variable into a five-category interval variable. The categories are: strong Democrat; weak Democrat *and* independent-leaning Democratic; independent; weak Republican *and* independent-leaning Republican; and strong Republican. While pointing out that "the interval assumption is a strong one which may not be strictly correct," he goes on to conclude: "Fortunately, the findings based on the three-category nominal scale of party identification generally agree with the findings using the interval assumption" (p. 16). No supporting evidence is offered. The acceptance of the analyses that follow, of course, depends upon the acceptance of the assumption made and the collapsing.

The foregoing problem aside, Knoke's findings are less than startling, not only because his well-placed articles cover most of his book but also because the ground was well covered in two recent and quite good longitudinal examinations using the same data—Gerald Pomper's *Voters' Choice* (Dodd, Mead, 1975) and Herb Asher's *Presidential Elections and American Politics* (Dorsey, 1976). Knoke's potentially interesting findings concerning very stable non-southern Protestant support of the Republican party, change in party support in the South, the effects of subjective class identification and occupation on party identification, and the use of congressional vote (in a section of one chapter) as a baseline from which to analyze presidential voting and realignment were explored in depth by this reviewer with Everett Carll Ladd, Jr., in our *Transformations of the American Party System: Political Coalitions from the New Deal to the 1970s* (Norton, 1975). Knoke's findings, generally, are consistent with what these other works have found.

In the treatment of "Party and Voting Behavior" two widely discussed themes were explored: party decomposition and voter realignment. As in many examinations preceding it, independent voters were shown on the rise; and, basing his argument on a cohort analysis, the author refutes the life-cycle explanation of this phenomenon. In fact, "It is to be hoped that the present analysis will lay the life-cycle hypothesis to its well-deserved rest" (p. 135). Also, straight-ticket voting and strong party identification were shown on a rapid decline. The three trends are phenomena of the mid-1960s. Relating party identification to congressional and presidential voting: "Neither . . . indicates a severe disruption of the capacity of party identifications to shape national voting choices within the electorate" (p. 145). Interestingly, in a regression analysis of the two-party presidential vote on indices of party identification and, with open-ended questions, candidate evaluation and partisan issues, issues were shown to be *unimportant.* In fact, "the importance of issues declined markedly from 1952 to 1972" (p. 153, emphasis added). Is this a refutation of important and expanding research? Realignment, a part of these analyses and that of the 1968 Wallace vote, was equated with expected change to a majority Republican party and reported "overly optimistic." The micro-level—"social bases"—analysis in this context would reveal a "conversion" (in terms of Pomper's "Classification of Presidential Elections," *Journal of Politics* (1967); one base of Democratic Party support was substituted for another in the decades since the New Deal. Ample analysis exists on this point.

As part of his brief conclusion, the author examined a "social indicators model" of voting with Republican and Democratic party support and unemployment and constant dollar change in gross weekly earnings—all in percentages—for the 1948–1974 biennial elections. The 1974 Democratic congressional gains were thus explained. Finally, who could fault Knoke's conclusion? He ends by saying, "The only safe prediction which can be made is that the change and continuity in American politics will continue to provide an endless source of fascination" (p. 162). Many readers, however, may well take exception with many of the assumptions on which the analysis rests.

CHARLES D. HADLEY

University of New Orleans

The Politics of Environmental Reform: Controlling Kentucky Strip Mining. By Marc Karnis Landy. (Washington, D.C.: Resources for the Future, The Johns Hopkins University Press, 1976. Pp. xiii + 400. $8.95, paper.)

With few exceptions, scholarly effort accorded the study of coal-related policy development has been minimal. Most energy-related policy studies have focused on the more "glamorous" fuels such as petroleum and natural gas. Recently, however, our gargantuan appetite for energy coupled with the realization of finite energy supplies has thrust coal, our most abundant fossil fuel, into the public spotlight. Those interested in issues resulting from the strip mining of coal, as well as those attempting to resolve these issues in government will find this an insightful and beneficial book.

In general, Marc K. Landy analyzes "the role of politics in environmental policy formulation by providing a detailed examination of one particular form of environmental regulation, strip mine control, in one state, the Commonwealth of Kentucky" (p. 1). More specifically, he directs his efforts to sorting out those factors instrumental in developing a strict strip-mine law on the one hand, while searching for an explanation for failure to enforce this law on the other. His concerns, therefore, extend beyond merely formulating a course of action, but also emphasize the processes of policy legitimation and implementation. His data base consists "largely of interviewing politicians, journalists, coal operators, regulatory officials, and community activists" (p. v). Although not primarily comparative, the largely non-quantitative analysis does contain a longitudinal comparative dimension contrasting gubernatorial administrations that have dominated Kentucky between 1947 and 1974.

The study provides exceptional insights into (a) the evolution of an administrative faction whose political autonomy made it possible to endorse strip mine control standards by the mid-1960s; (b) the New Deal philosophy that provided the rationale for attitudes toward government activity within this faction; (c) the political, economic, and social context that provides the setting for the uncommonly broad powers found in the gubernatorial office; and (d) the politics involved in formulating, legitimizing, and implementing the 1966 law. Landy alleges that "the success of . . . the strip mine control program . . . was directly attributable to the ability of a specific political organization to create and maintain a high level of political autonomy" (p. 15). Consequently, this autonomy enabled strip-mine policy development to occur in a state whose economic welfare depends heavily on coal-related revenues. Landy further points out that once this "political organization," consisting of Governor Breathitt and his advisors, was removed from office in 1967, the enforcement philosophy returned to that of "minima as maxima." This point supports advocates of national reclamation standards who argue that states either do not or cannot enforce their reclamation laws.

This work undoubtedly has value for those studying governmental concerns with public problems. In the first place, public problems reach the policy agenda in a variety of ways. In this case, the most crucial actor (Governor Breathitt), a product of the New Deal, perceived and defined a public problem (detrimental effects of strip mining) and proposed a course of action to resolve this problem. This was accomplished without pressure from the public or organized interest groups; in fact, the most intense outbursts came from coal representatives, something not to be taken lightly in Kentucky. Yet Breathitt's own attitudes, his leadership qualities, plus the powers of his own institutional base prevailed in 1966. Contrast this to the case of West Virginia where efforts to develop compensation for black lung victims in 1969 required the combined weight of a major mine disaster, a three-week wildcat strike, national publicity, and the dedication of outside reformers.

Second, once a proposal becomes law, the difficulties for proponents do not end; in fact, the implementation process provides another opportunity to inhibit policy intentions and actions. In short, support needs to be built and nurtured throughout the entire policy process, not only when policy is being developed. Third, this study depicts the variability of institutional arrangements in our federal system which affect the options available to policy makers. Finally, as Landy points out, policy analysts need to address "the role of human excellence and human frailty in determining . . . outcomes" (p. 344).

The book's major strength lies in its meticulously researched description and analysis. As one who is quite aware of the murky nature of analyzing the policy process, the study is in need of a bit more conceptualization, especially in chapters 1 and 11. A more thorough literature review and greater effort at generating more precise research expectations would have been helpful. There is no doubt, however, that this study is also of great use as supplementary reading material for courses in public policy, especially with a state level focus.

DIETER MATTHES

Francis Marion College

Bureaucratic Insurgency: The Case of Police Unions. By Margaret Levi. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. ix + 165. $15.00.)

This brief volume is a useful addition to the literature of municipal employee unionism primarily because of the detailed histories it presents of the developing strength of three police unions, in New York City, Detroit, and

Atlanta. These accounts, which occupy three-quarters of the book, are thoroughly researched and documented and are presented with sympathetic understanding of the needs and problems of both the city officials and the union leaders. The author's interview data yield interesting insights into conflicts and compromises in all three cities, with political and bureaucratic strains clearly depicted. In each case the organized police officers had to overcome the determined opposition of their police commissioners to really establish themselves as unions (in New York and Atlanta) and to win a tangled pay-and-grievance dispute (the famous 1967 "blue flu" episode in Detroit). The narratives are straightforward, clear, and even vivid for the most part. Each one is flawed, however, by instances of doubling-back historically, so that readers have to stop and find out where they are.

In the analytical material that precedes and follows the case studies, the author has chosen to present the police union members as *insurgents*. This will be too strong a term for some tastes, including my own. Although the three unions become adversaries of their commanders, in a labor relations sense, and although two of them took job actions of questionable legality, it is still a strain to see them as revolutionaries. Nevertheless in the final chapter the author draws on writings by a Marxist and a neoclassicist to support her view that public employee unions are a threat to the fiscal soundness of local governments and therefore to such governments' ability to provide a helpful economic environment. She writes:

> To some extent ... collective action by public employees contributes to the crippling of capitalism and the capitalist state. The militance of municipal workers intensifies the urban fiscal crisis, undermines the conditions of capital accumulation, and delegitimizes government in the eyes of the citizenry. Such a formulation is correct as far as it goes, but it is certainly incomplete. It fails to take account of the failure of the public employees to build a social movement in any way concerned with the transformation of fundamental economic and political structures. And it fails to come to terms with the counterstrategies of urban officials....
>
> Despite the havoc and fiscal crisis that militant state sector unions help to create, they are still very much subject to the common mechanisms of social control exerted by the capitalist state....
>
> The major success of the counterattack has been its ability to deepen long-standing social divisions. The shrinking economic pie has essentially forced different factions of the working class to fight each other for necessary but scarce resources (pp. 154–56).

That is certainly one way the scene can be painted. It would seem simpler, more consistent with the case study material, to present the events as three among many political contests among many interest groups for advantage and resources. Police officers have always had political connections and have used them in competition with others. Over the years they have increased the effectiveness of their organizations, have adopted new tactics, and have improved their position relative to other contestants. As public-sector labor relations mature on both sides the contest will be no less vigorous and the results no less expensive, but not as threatening to other citizen groups (or to capitalism) as this book suggests. The political counterforces will keep the effects from being truly crippling. Urban fiscal troubles, moreover, have resulted primarily from economic decline and demographic change, and public employee unions should not be singled out for major blame.

DAVID T. STANLEY

Vienna, Virginia

Too Little But Not Too Late: Federal Aid to Lagging Areas. By Sar A. Levitan and Joyce K. Zickler. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. ix + 172. $14.00.)

Can the national government develop a policy that would contribute significantly to the economic development of "depressed" areas? Levitan and Zickler provide assistance in answering this question by reviewing and analyzing the past 15 years of federal experience in aid to depressed communities and the Appalachian region. The story, as the authors put it, is "a tale of attempting to satisfy a Gargantuan appetite with Lilliputian doses" (p. ix).

The authors, who are economists, do not explain this sad tale in economic terms; rather they employ political and philosophical explanations. They note, for example, that it has been a long-standing tenet that the federal government should not interfere in private business decisions by making loans or loan guarantees. Such policies only prop up marginal businesses and thus undermine the free, competitive market. The authors note, however, that federal policy has steadily moved away from this position. The Area Redevelopment Act of 1961 used the business loan as its cornerstone; the underlying assumption was that depressed areas suffered from a lack of venture capital.

Growing discontent with the implementation of the Area Redevelopment Act resulted in the more comprehensive Public Works and Economic Development Act of 1965. This act established a substantial public works program which, in time, dwarfed the business loan as the mainstay of federal aid to depressed areas. Public works programs, of course, are well-publicized political tools for combating unemployment. Their contribution to long-term economic development is debatable, however. The authors argue that the 1965 legislation was like other economic development programs "a half-hearted effort–a cross between welfare and economic stimulation–to help lagging areas" (p. 3).

The Economic Development Act was initially conceived to aid depressed areas. Ever-expanding eligibility pressures, however, have resulted in spreading available funds over half the counties in the United States. The authors maintain that "the Economic Development Administration has practically ceased to be a program focusing aid in depressed areas" (p. 56).

A similar view is taken of the Appalachian Regional Commission. This program was originally targeted at the Appalachian heartland but was extended to cover parts of 13 states. Nonetheless, Levitan and Zickler argue that the commission has become a successful political force which, over time, may stimulate economic development in Appalachia.

This book should be read by political scientists who would like an overview of economic development policies since 1961. The authors do a fine job highlighting various policies and programs. The most informative chapter, however, focuses on the administration of business loans under the Public Works and Economic Development Act. This chapter details the decision practices and priorities of the Economic Development Administration.

All things considered, the message contained in this book is not optimistic for those of us living in "lagging" areas. The authors' analyses do not support the contention that a national policy can be developed to stimulate economically depressed areas.

DONALD C. MENZEL

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Poliscide. By Theodore J. Lowi and Benjamin Ginsberg et al. (New York: Macmillan, 1976. Pp. x + 306. $5.95, paper.)

Urbanologists know much of the birth and life of cities, but the literature is almost bereft of knowledge of their death. Some analysts have recently noted the death of public agencies, a few in education have studied school consolidation, but the politics of urban decline and disappearance is almost totally ignored. Into this unknown land have wandered Theodore Lowi and his associates to report on a small town, Weston, Illinois, which was extinguished in the 1960s by the layers of public and private power above it. Like the well-utilized case study, this one both illuminates familiar propositions and also raises new propositions for later analysis. Moreover, it seeks to demonstrate that what occurs when "public interest" is defined not holistically but in terms of each official's "fulfillment of obligations to serve his clientele" (p. 16), a theme for which Lowi is well known.

The facts are that nuclear scientists sought a location for a nuclear accelerator; federal elected officials and administrators selected Weston (outside Chicago) for reasons primarily political; state and county governments eagerly accepted this federal bonanza; and all these policy makers agreed to remove the small group of Westonites to make way for the installation, with even many of the latter agreeing to their own loss of home and location. These events are described in *Rashomon* style, from the perspectives of all participants. This account is accompanied by an inventory of the motives, strategies, and payoffs of those involved.

In the process one learns much about one type of federal policy making and implementation which has characterized the writing of those either disappointed by what they regard as "failures" of federal efforts to deal with social problems or by those who know a scholarly trend when they see one. The type of federal interaction studied here is not one that seems to fit Grodzins' concept of the sharing of functions and resources. Lowi et al. declare this concept is not typical of federalism, anyway. Rather, "the typical case, and that includes the building of the accelerator, is one in which the federal government authorizes a project or an activity and then leaves all of the details to the personnel in the field and to the local officials and interests concerned with that activity or project. And in these instances, as all of our cases show in various ways, the relationships among the layers are relationships of mutual noninterference and mutual ignorance. The outcome of this is one in which the periphery usually exploits the center" (pp. 27–28).

Such federal policy interaction was first popularized by Pressman and Wildavsky, of course, and most recent scholarly papers work hard to validate the proposition. There is no question that many federal interactions can be characterized in this fashion by delegation, dilution, and diversion. But there are several reservations which need be laid against this popular interpretation of American federalism. Not least of these is the substantive analysis by Sar Levitan and Robert Taggart in *The Promise of Greatness* (1976) of the Great Society's social welfare programs. They conclude that these laws were far more effective and had far less negative externalities than is acknowledged by current received wisdom. We may be in the position of deciding whether the cup is half full or half empty, but currently a form of "groupthink" has fastened upon only one interpretation.

Regarding this death, or "poliscide," that Lowi and his associates report, I have several reservations as to what was really demonstrated. When so many agents and levels of government supported this particular program, including large numbers, maybe even a majority, of Westonites (Ch. 10), when there has been agreement horizontally among the national interest groups and the Washington public forums and agreement vertically among the federal levels—in short, when there has been evidenced so much agreement reaching almost consensus, why do the authors define the extinction of Weston as not in "the public interest" and decry the lack of a holistic vision? When one has the vertical consensus displayed here, how can one argue that some federal policy is eviscerated by diffidence and opposition at state and local levels? When Dillon's Rule is still good constitutional law in the 50 states, why give the impression that the use of state and county authority to wipe out Weston for the accelerator is somehow improper?

In short, the argument that federal goals for national welfare can be dissipated by localism—not new but frequently clearly applicable—does not adequately apply to the facts of this Weston case. There are quite serious questions about what happens to citizens' rights and property in a society which is increasingly centralizing authority at state and national levels. But there is also the serious question whether a counterbalanced centripetalism can go unchallenged when it results in infringements on the rights and properties of others. It just is not clear in the Weston case that the total pie of American rights was grievously divided.

Further, the concept of federalism that the authors seek to typify in Weston ignores the other varieties of intergovernmental actions. There are numerous occasions when Washington actually coerces local systems to meet national goals with great success, e.g., southern school desegregation (see Harrell Rodgers and Charles Bullock, *Coercion to Compliance* [1976]). There are also many occasions of shared interests mutually pursued, and yet other patterns of "cooperation and conflict" which in recent years have witnessed much public policy and research. But the evidence of Oakland and Weston are not the only cases one must comprehend in order to develop a full-bodied theory of American federalism.

While I fear the Weston case may not bear the weight of the larger formulation it seeks to carry, there are many useful analytical approaches in this work—how ambiguity in federal law encourages centripetalism, the politicization of the science establishment, how the absence of a sense of public interest generates pork-barrel decision making, how local planning law can be used to foster socioeconomic purposes in suburbia, how eminent domain can be used for political purposes, why participants may be content with limited information about events (it seems satisficing is everywhere), and the consequences for our own discipline of specialization in the fact of a total political process. These scholars have much to say that is wise and even provocative. It is at this conceptual level that the work is most valuable, despite the reservations earlier expressed about its use of the theory and practice of federalism. As always, in any work associated with Lowi, the writing is lucid and engaging. Ultimately, though, this small work may be most useful for directing us to something other than studies of stasis and change, that is, to the significance of the demise of governments in a broader theory of system behavior.

FREDERICK M. WIRT

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The Politics of Administrative Representation. By Dale Mann. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xiii + 175. $17.00.)

Dale Mann's book is about a concept which, from the point of view of traditional democratic theory, is troublesome. However, from the point of view of educational administration, the concept is realistic and the book welcome. In essence, the book explains the representative roles and behavior of 165 school administrators (principals and superintendents) in New York State. Although there are some excellent chapters of an advisory nature (that is, containing recommendations to administrators and citizens about the structure and possibility of participation), political scientists will be most interested in the descriptive portions.

Why should administrators represent anybody? With regard to educational policy making, Mann quite simply states his case: "There was a time when citizen boards of education did, in fact, run schools.... But as the power of professional school administrators grew, that arrangement became more and more unsatisfactory, and administrators seized more and more de facto autonomy in both policy determination and implementation" (p. 5). Thus, administrators as the effective policy makers are legitimate objects of inquiry as to how they go about the job of representing constituents, or failing to do so.

To establish the distribution of representative roles, Mann uses the traditional "trustee," "delegate," "politico" distinction. Most administrators (61 percent) are trustees. Such a finding is hardly surprising. Most state legislators, city council members, and school board members are also trustees. With the majority of all public decision makers believing they should follow their own judgment, one wonders if the widely documented public apathy is nothing more than common sense. In any case, what is intriguing is not the fact that school administrators, appointed officials, are trustees. Of considerably greater interest is that they are no more so than are elected public officials. One would assume—as earlier studies concluded—that administrators would find a "delegate" role quite incompatible with their image of their job. However, Mann's data suggest that, since administrators are, in fact, representatives, they behave like typical representatives.

Thus, administrators, who actually "run" the schools, are as "responsive" as the school boards, which, in most cases, legitimate administrative decisions. I wish Mann had made more of this point, but the style of the book is one which requires a close adherence to the limits of the data (a laudable tendency, of course).

Even though most administrators are trustees, there are variances in response style, some associated with individual characteristics, others associated with community and school characteristics.

Not surprisingly, older administrators are much more likely than their younger colleagues to be trustees, usually because they believe their experience lends credence to their claim for expertise. However, increased experience *at a particular location* was not associated with response style. Age alone made a difference. Will the younger administrators (56 percent of whom are trustees) become more aloof as they grow older? Presumably so.

As corroboration of this finding, administrators whose predecessor was discharged or transferred involuntarily are less likely to be trustees. It is unfortunate that the small size of the sample prevented Mann from controlling for age and the fate of the predecessor. Are younger administrators assuming the duties of a disgraced predecessor even less likely to be trustees? The possibility becomes even more tantalizing when we learn that younger administrators are more politically ambitious. What would we learn about response style if we examined young, ambitious administrators who succeed those with an unfavorable fate?

To carry the puzzle further, superintendents (positions to which ambitious principals presumably aspire), are more responsive than principals. This suggests the possibility that movement toward more visible positions increases responsiveness, even if the position is further (at least physically) from clients. Principals, of course, are tenured, while superintendents are subject to dismissal by school boards. Principals, according to Mann, actively have more decision latitude than do superintendents. Then why want to move up, unless one is willing to modify one's response style? But, the older one becomes, the less responsive one becomes. Again, we need to know more about the interaction of the variables.

The above is not meant as a criticism of Mann's analysis, but rather as a suggestion for additional explication. If the data were not interesting and well reported, there would be no point in pursuing the matter further.

Response styles also vary with communities. Homogeneous attendance areas attract trustees; heterogeneous areas do not. The more administrators perceive a plurality of interests, the more likely they are to adopt a delegate style. However, in reality, such administrators rely almost exclusively upon "permissive voluntary groups with little effect on their decision (PTA's, for example)" as channels of communication (p. 55). In fact, less than one-fourth of the communities examined by Mann had achieved an organizational climate beyond that of PTA's and other harmless groups. However, in such communities are found the fewest trustees (55 percent). Since, even in organizationally active communities, the trustee role dominates, Mann concludes, "most school administrators strive for autonomy although they recognize that it is important to be verbally committed to democratic participation" (p. 55).

There are, of course, school boards. According to Mann, "only" 37 percent of the administrators would reject a direct order from the school board. Further, in the event of a dispute, the board is the last place that administrators would turn for support (perhaps, as Mann suggests, because the board might be the source of the dispute). In any case, who are these administrators who would reject a direct order from a legally elected public body?

Obviously, my comments indicate that I

regard the book as provocative and well argued. There is another book waiting to be written, if not with these data, then certainly by expanding upon Mann's insight into the administrative process.

HARMON ZEIGLER

University of Oregon

Rulemakers of the House. By Spark M. Matsunaga and Ping Chen. (Urbana: University of Illinois Press, 1976. Pp. xv + 208. $7.95.)

A study that combines the skills, vantage points, and approaches of an active elective official and a political scientist holds much promise for both the casual reader and the serious scholar. Participant politicians can be the source for rich information about and for interpretation of events that have transpired in their arenas of operation, and political scientists can offer skills in analysis of data, integration with existing research materials, and maintenance of objectivity to make the collaborative effort a significant contribution to the available literature. This seems to be what Senator Spark Matsunaga (D-Hawaii) and Ping Chen had in mind in undertaking a new study of the House Rules Committee on which Matsunaga served from 1967–76. The end result, *Rulemakers of the House,* although a less than perfect marriage of the two perspectives, does offer some useful insights for students of the House.

It is important to realize at the outset that this book is not merely a puff for Matsunaga. No doubt its release during his campaign for the Senate was not unplanned. Many of the examples do refer to Matsunaga's role in a positive light. (Matsunaga returns from Hawaii to cast a tie-breaking vote to send the land-use planning bill to the floor.) Some discounting on the reader's part is necessary. However, the main focus of the book is not on Matsunaga but on the changing nature of the Rules Committee. This book is not just the reporting of Matsunaga's recollections. The authors interviewed 59 House members from 1970 to 1974 (including Rules Committee members and party leadership) in the preparation of this study. They also spoke with three chairpersons of the Rules Committee, Howard Smith, William Colmer, and Ray Madden. In addition, they collected substantial data about the committee on subjects ranging from discharge petitions filed against the Rules Committee, to rules defeated on the floor, to party unity scores of committee members prior to their appointment, to levels of party agreement on roll-call votes in the committee. These data are contained in tables and appendices at the end of the book and comprise nearly a quarter of the book's total length.

The authors analyze a range of topics about the Rules Committee. Their primary focus is the tension on the committee and among its members over whether to reflect the will of the party leadership or to behave independently. Matsunaga and Chen reflect this tension in chapters discussing the sanctions that the leadership and the House may use against the committee; the qualifications for Rules membership and the way members are recruited; the goals of the committee's members in terms of influence and prestige within the House and constituency and reelection benefits (but not, unfortunately, in terms of assisting the House in making policy choices); the changing role, power, and influence of the Rules Committee chairperson from 1955–74; and committee decisions as reflecting differing relationships between party leadership and committee members. To deal with these subjects and several others in less than 150 pages of text is no small undertaking. Although a good deal of information is packed in, one finds that many points need substantial elaboration, support, and refinement. Thus, for example, an early chapter which delves into the "rule-making powers" of the committee spends only slightly more than a page discussing the use of the Rules Committee in settling jurisdictional disputes among House committees. Yet given the overlap of committee and subcommittee jurisdictions, the need to settle jurisdictional disputes has become a significant source of Rules Committee influence in the House and reflects the changing nature of the committee and the House.

The limited length of the book also cuts into its richness. One should not expect a wealth of new anecdotes or some previously hidden truths to be revealed. But given the access the authors had, one is struck by the feeling that this is not an "insider's" account. Instead one senses that the authors were constrained in their use of supportive stories.

Perhaps the chief misgiving I have about the book relates to a certain bias in its coverage. It is difficult for me, and I suspect for most students of Congress, to accept a study of the Rules Committee during 1955–74 as complete without substantial discussion of the role Richard Bolling has played in the operation of the committee. Matsunaga and Chen never really discuss Bolling's leadership role on the committee and his role as spokesman for Democratic House leadership. They do occasionally draw on their interviews with Bolling, but they never come closer to recognizing his role than to note: "Even if the leadership has a 'personal representative' on the committee (e.g., Richard Bolling and Homer Thornberry for Speaker Sam Rayburn and Thomas P. O'Neill for Speaker John McCormack) who serves as the leadership's eyes and ears. . ." (p. 101). The facts remain that the role has been more than just "eyes and ears," and Bolling has served in that capacity, with the exception of a brief period, throughout the time the authors are studying. In part the authors may be confusing the committee chairmanship with committee leadership, and in part it may reflect their preference for a "semi-independent" Rules Committee rather than one which is more closely tied to the party leadership.

Despite these shortcomings, Matsunaga and Chen do add important information about the Rules Committee. The data they have gathered is very useful, and their efforts to use the data for preliminary tests of hypotheses about the committee are valuable. In addition, by meshing the skills of the political practitioner and the political scholar, they do avoid some of the pitfalls of the separate approaches.

BRUCE I. OPPENHEIMER

University of Houston

The Conservative Intellectual Movement in America: Since 1945. By George H. Nash. (New York: Basic Books, 1976. Pp. xv + 463. $20.00.)

This book is a timely addition to American intellectual history. By surveying the contributions of a large number of conservative American thinkers Nash has provided a useful summary of post-World War II conservatism. The narrative is thoroughly documented with 100 pages of footnotes and bibliographic information. Nash's chronicle of conservative thought aims at establishing such thought as a legitimate form of intellectual activity within the American culture. His approach is sympathetic, but largely descriptive.

Probably because of the variety of individuals and ideas with which he must deal, Nash at the outset specifically declines to provide a definition of conservatism. From his general perspective Nash sees post-war American conservatives as falling into three camps with considerable overlap and interaction among them. Originally the libertarian supporters of individual freedom from government were largely concerned with economics and received their impetus from Friedrich A. Hayek and Milton Friedman. In contrast to the libertarians, the traditionalists, led by Russell Kirk, Richard Weaver, and to a lesser extent, Leo Strauss and Eric Voegelin, found the answers to the human condition in a western heritage that rested on values antecedent to and often opposed to those engendered by the Industrial Revolution. Perhaps the most famous spokesman for the conservative anti-Communist element was Whittaker Chambers, with James Burnham, William F. Buckley, Jr., and L. Brent Bozell also important in this respect. Despite the vagaries that are likely to appear whenever one considers a large number of thinkers, Nash is successful in presenting postwar intellectual conservatism as an identifiable school of thought whose spokesmen have contributed to American ideas about social life and public policy.

Much of the coherence that has existed in postwar conservatism can be attributed to the efforts of Kirk, Buckley, and the late Frank S. Meyer. Russell Kirk's *The Conservative Mind* was important to the formulation of traditionalist conservatism; through his founding of the journal *Modern Age,* Kirk provided an outlet for scholarly conservative writing. Nash describes Buckley's *God and Man at Yale* as "probably . . . the most controversial book in the history of conservatism since 1945" (p. 30). Additionally, Buckley established the *National Review* and hosted the founding meeting of Young Americans for Freedom at his home. Meyer's importance to conservatism stems in great part from his efforts to bring greater unity to the movement. Although he was firmly situated in the libertarian camp, Meyer in the early sixties attempted a "fusion" of the traditionalist and libertarian camps by arguing that individual freedom and human virtue are inextricably linked. Meyer's efforts did not resolve what remains a fundamental difference between libertarian advocacy of individual freedom and the traditionalist need for social order, but Nash believes that he did provide the basis for a working compromise between the two camps.

Nash sees the conservative movement as achieving substantial success in terms of public recognition of its legitimacy by the late sixties. By this time the questionable defense of Joseph McCarthy and his tactics and the opposition to *Brown* v. *Board of Education* that some conservative intellectuals had espoused had become dead issues and in his bid for the presidency Barry Goldwater had provided a public platform for conservative ideas. Just as importantly, campus unrest and the excesses occasioned by opposition to the Vietnamese War fostered increased sympathy for conservative positions. Finally, the large spending programs engendered and maintained at the national level by liberal-oriented administrations were beginning to be subjected to scholarly criticism that often seemed to support previous conservative warnings.

The book is well written and it will undoubtedly stimulate many to delve more deeply into the thought of American conservatives. Nonetheless, Nash's unwillingness to move far from simple description of ideas and of the channels used for their propagation will make his approach appear unnecessarily superficial and even misleading to many social scientists. In particular, Nash's lack of cultural and conceptual elaboration contributes to what appears to be some confusion on his part as to the relation of majoritarianism to conservative doctrine generally. It is difficult, for example, to understand the large amount of space given to Willmoore Kendall, unless this extensive treatment is to be seen as an indication of the importance Nash places on Kendall's attempts to provide a majoritarian basis for conservatism. At another point, Nash states that it was natural for conservatives during the 1950s and 1960s to be antimajoritarian because they were in the minority (p. 218). In contrast to what seems to be Nash's attribution of historical coincidence to conservative beliefs and to his apologetic stance regarding the minority status of conservatism, one strongly suspects from the material that Nash himself presents that most conservative thinkers would not retreat from the position that conservatism is a system of postulates that stand on their own merits regardless of majority support and transient historical circumstances.

ROBERT A. HEINEMAN

Alfred University

Party Dynamics: The Democratic Coalition and the Politics of Change. By Richard L. Rubin. (New York: Oxford University Press, 1976. Pp. x + 203. $9.95, cloth; $3.95, paper.)

Party Dynamics is divided into two distinct parts. The first analyzes mass political attitudes and voting behavior, the second studies elite politics within the Democratic party. Both parts provide some useful evidence and some provocative arguments.

Rubin's study of mass attitudes attempts to assess the effects of black migration to the cities, and of white migration from the cities to the suburbs. Rubin draws upon Michigan Survey Research Center data, CBS surveys, and surveys by Free and Cantril, but his most extensive and original analyses are based upon Harris surveys. Several conclusions emerge from Rubin's analyses. Migrations to the suburbs have had little or no effect in converting Democrats to the Republican party, and the effects of migration have been negligible among both Catholics and trade unionists. There are, however, regional differences in voting behavior. Trade unionists in the East are more likely to vote Republican than are trade unionists in the Midwest—mainly, Rubin reasons, because Republican leaders in the East have been more liberal than their midwestern counterparts. On the other hand, eastern Republicans are more likely to vote Democratic than are Republicans in the Midwest. Class-based political cleavages, Rubin argues, are declining in the East, but persisting in the Midwest.

Rubin justifies his focus on intraparty competition among the Democrats on the grounds that major conflicts are fought out within the dominant party. Precisely because Democratic migrants to the suburbs have retained their party loyalties, they have increasingly challenged the urban political machines for control of the Democratic party. Changes in delegate selection procedures, Rubin argues, shift the advantage to new elites that represent middle-class Democratic suburbanites. As blacks become politicized, they, too, are making demands within the Democratic party, but their demands have led to "fierce counter-mobilization" among working-class whites. While coalitions may develop between white liberals and blacks, Rubin argues that such coalitions will be unstable, because white reformers can afford to press their principles at the risk of electoral defeat, whereas blacks have more immediate social needs that make defeat more costly. "Until significant headway can be made in assuaging local racial conflict among Whites and Blacks in the critical urban regions of the North," Rubin concludes, "the Democrats will remain a deeply vulnerable and unstable coalition" (p. 177). In solving their problems, Rubin argues, the Democrats cannot afford "the luxury of full-blown intra-party democracy," but need leadership that will select candidates and issues likely to win elections.

Rubin's analysis provides useful insights, but his public opinion analyses are quite limited. The finding that suburban migration seldom leads to changes in party affiliation has been well documented, although Rubin brings new data sources to bear on this question. The analyses of regional differences exclude the West, and, more importantly, the South, where the most dramatic changes in presidential voting behavior have occurred. But the most limited aspect of his analysis is that Rubin attempts to study change without using time-series data. He restricts his secondary analyses to surveys conducted between 1968 and 1972, even though there are relevant data for the entire postwar period.

While Rubin's public opinion analyses are narrow in scope, his analyses are sound, and his interpretation of the data is reasonable, and, at times, insightful. Moreover, Rubin's discussion of intraparty elite competition is rich in historical material, and his discussion of "Race and National Democratic Politics" is one of the best analyses of this subject. While his conclusions are somewhat elitist, he has nonetheless identified important problems that Democratic leaders must resolve.

PAUL R. ABRAMSON

Michigan State University

The United States Congress in Comparative Perspective. By John E. Schwarz and L. Earl Shaw. (Hinsdale, Ill.: Dryden Press, Holt, Rinehart, and Winston, 1976. Pp. 421. $13.95.)

This book has a laudable aim: to examine the operation of the U.S. Congress by comparing it to the British House of Commons, the French National Assembly, and the West German Bundestag. A premise of the work is that one is better able to analyze Congress, both in the sense of building empirical theory and evaluating congressional performance, through comparisons. The book is an admirable and useful synthesis of diverse pieces of literature. With a few exceptions, it is not a work of original data analysis nor of development of concepts with which legislative scholars have been heretofore unacquainted.

The introduction and chapter 1 set forth a theoretical overview, a discussion of the major concepts to be used in making the comparisons among the legislatures. Role theory, incentive theory, theories of power, and systems analysis are all nicely summarized. The authors rightly argue that legislative behavior can be understood as a joint result of the characteristics of legislators and of their environment. Then follows a chapter reviewing the structural features of the four legislatures, and a chapter presenting a set of case studies of selected pieces of legislation, largely in simple narrative form, without much attempt to generalize from the cases or to be analytical about them.

The analytical core of the book, in my judgment, is in chapters 4–10, particularly chapters 4 and 8, for that is where the authors attempt to explain why the differences among the legislatures exist, as opposed to simply describing their features or discussing what theories might conceivably be useful. Two chapters concentrate on the place of political parties. Given that all western democratic legislatures are party-structured in varying degrees, this is clearly an appropriate beginning. Several possible explanations for differences in the prominence of party are discussed; the most persuasive ones involve the members' career ambitions, coupled with features of the various political systems which make the achievement of those ambitions more or less contingent upon party loyalty. The discussion of the individual legislator's ambitions is more fully elaborated than the consideration of the environmental or structural elements; however, it is possible that the structural factors are at least as important, if not more so, than the individual. At various points, the authors resort to demographic indicators as surrogates for attitudinal information, which is unfortunate but perhaps inevitable, given the nature of the work.

In their three chapters on legislative-executive relations, the authors first argue, persuasively in my opinion, that the image of executive initiation and legislative reaction is seriously overstated in much of the literature, as applied to all of the legislatures under examination. In general, the German and American legislatures are portrayed as more influential vis-à-vis the executive than the French or British, particularly on the formulation of domestic legislation. There is also a descriptive chapter on legislative supervision of executive action, which summarizes the tools at the disposal of legislatures and discusses the limits on their use. The major theoretical contribution concerning legislative-executive relations comes in chapter 8, in which it is argued that the differences among the countries in the strength of the legislature relative to the executive are the result of differing cohesion of the executive's party and the differences in the independence of the legislative committees, the latter partly because a system of independent committees generates a greater information capacity for a legislature. An alternative possibility, I suppose, is the causal reverse: that in some parliamentary systems, a widely shared desire to insure strong leadership centered in the majority party and cabinet results in a rather deliberate weakening of committees by designing shifting jurisdictions and memberships, rather than having weak committees contribute to strong central leadership.

Chapters 9 and 10 examine legislative-interest group and legislative-citizen opinion relations, respectively. The discussion of interest group strategies should be quite familiar to those acquainted with the literature; and is rather loosely structured around a couple of dimensions: direct vs. indirect strategies, and recruitment vs. access once in office. By and large, examples of interest group strategies are simply presented illustratively, without being used to compare the countries systematically.

Chapter 10, on representing constituency opinion, starts with a discussion of the party responsibility argument, on the grounds that the responsible parties model is a model of representation alternative to the congressional model. In the process of attacking the question in this fashion, a much interesting literature on representation (e.g., Dexter, Fiorina, Mayhew, and other works) is unfortunately either drastically downplayed or ignored. I happen to agree with the authors' conclusions, that particularly the U.S. House of Representatives is quite representative of constituency opinion, and that minorities have a voice in the Congress greater than their simple demographic presence would indicate. But in this section, one of the primary attempts in the book at original data analysis, their methodology is highly problematic. Measures of the public's self-identification (liberal or conservative) are related to an ADA-ACA congressional index without convincing evidence that the two dimensions are comparable; legislative "liberals" are defined as those having a higher ADA than ACA score and vice versa, which lumps 55–45 liberals together with 95–5 liberals; and central indexes in the analysis are suspect because of arbitrary weights attached to the raw values.

Students and active scholars both can learn a great deal from this book. The authors have skillfully brought together an extremely diverse and scattered literature, which is a considerable contribution to the process of building a body of findings and concepts concerning legislative behavior.

JOHN W. KINGDON

University of Michigan

Economic Growth and Neighborhood Discontent: System Bias in the Urban Renewal Program of Atlanta. By Clarence N. Stone. (Chapel Hill: University of North Carolina Press, 1976. Pp. xv + 256. $13.95.)

For those unfamiliar with Atlanta, the city is a billboard creation, celebrating classic images of local vitality, economic growth, and progress. The reality behind such imagery is seldom exposed, but political scientists who have read what their colleagues have previously written about Atlanta and other cities are likely to assume that the reality of Atlanta fits comfortably within the dominant categories of thought they have developed to comprehend city politics. The familiar descriptions of a "pluralist" politics often associated with downtown redevelopment include politicians who "broker" conflicts between various organized interests, temporary coalitions of power, and occasional concessions to voter sentiment. In this well-conceived and well-written study, Clarence N. Stone declares his own emancipation from these familiar assumptions and asks us to share his vision of a very different reality by accepting his "revisionist" view of community politics.

The occasion for Stone's investigation appears to have been his observation that a variety of neighborhood interests had been largely ignored in Atlanta's march toward a renewed central business district. To be sure, new hotels, new office buildings, a new civic center and a new downtown stadium were all built, providing the material for "billboard Atlanta," but in the process a full one-seventh of the city's population was displaced by government action, some neighborhoods "were simply demolished to make way for commercial and other forms of non-residential development" (p. 3), and a variety of new social problems were created, including a hardening of the lines of racial and economic segregation. Was this a result of the absence of neighborhood opposition? On the contrary, Stone shows that housing and neighborhood conditions were major issues in Atlanta, that neighborhood organizations repeatedly made demands—sometimes through confrontation—for alternative policies, and that newspapers frequently supported these demands. Were these demands ignored? Not at all. Meetings and hearings were held and concessions were made in some cases. Did a hidden "elite" of businessmen and industrialists simply impose their policy preferences on second echelon people occupying public office, as Floyd Hunter suggested? Again, the answer is no. Public officials were dominant actors and members of the economic elite often disagreed among themselves on specific proposals. What then, is the explanation for a redevelopment policy that systematically neglected the housing and social needs of neighborhoods as downtown renewal proceeded?

Stone's answer amounts to a thorough dismantling of pluralist ideology. He insists that the appropriate focus of inquiry is "policy" rather than "decision," and he makes clear that policy can only be comprehended over time, within an institutional as well as social context. These simple but powerful organizing ideas lead Stone to conclusions that require a different conceptual structure. Consistent acceptance of business demands and rejection of neighborhood demands during the period 1950–1970 is shown to have arisen from the "positional advantage" of business representatives, which in turn is related to "reformed" political institutions that systematically hampered the representation of neighborhood interests. The positional advantage of business interests—mayors and other public officials are recruited from business organizations, support for or opposition to policy choices are generated by business-dominated civic groups—permits those interests to concede on minor issues while controlling all major choices, to make symbolic decisions that are never implemented, and to conceal strategy in a series of minor technical choices whose overall pattern is often unassailable because it is hidden from public view. Maintenance of this positional advantage is thus actively pursued; successful pursuit gives the Atlanta political structure a stability not easily changed and which results in a long-term "system bias" in favor of business, rather than neighborhood interests. This is a far less naive and more subtle explanation than "pluralist" analyses typically produce, and if it is valid, says Stone,

> the basic premises of the pluralist theory of community power and policy formation must be revised. Electoral competition is not a guarantee of official responsiveness to constituency groups on policy matters. Political resources are not widely disbursed in combinations that enable groups to influence the policy making process. While "slack" resources (such as time, energy, and initiative) can easily be brought to bear on particular decisions, most outbursts of political activity can be met with temporary concessions, diverted, or waited out (p. 202).

Stone's work is thus as rich in theoretical significance as it is in the details of modern urban politics. More than any of its "pluralist" predecessors, it deserves to be taken seriously by those of us concerned with serious scholarship on matters of significance. Although not without defects—I doubt, for example, that Stone's "revisionism" is adequate to deal with the intergovernmental aspects of local power—Stone's work is a major and mature rethinking of the received wisdom about big city politics. Atlanta will never seem the same again.

THOMAS J. ANTON

University of Michigan

Party Image and Electoral Behavior. By Richard J. Trilling. (New York: Wiley, Interscience, 1976. Pp. ix + 227. $14.95.)

The study of electoral change is a growth industry. The eternal verities of the first two generations of election studies are being reexamined. At latest count there are about two dozen books and articles which refute, support or add to previous conclusions about the attitudes and behavior of the American voter. The principal reason for buying this latest entry is that its data cannot be duplicated for less than the purchase price of the book.

Party Images and Electoral Behavior rests on the thesis that the qualities the electorate uses to characterize the Democratic and Republican parties are important because: (1) in their frequency and variety they indicate "the extent to which the parties continue to arouse interest and concern"; (2) they may offer an especially reliable measure of the "salience of particular issues"; and, finally, (3) they may have "direct relevance for the theory of electoral realignment." Trilling employs several measures of party image but they are all based on the open-ended party master code. The first few chapters describe the basic measure, present its relative distribution over the past six presidential elections, and indicate its relationship to party identification and the vote. Subsequent chapters offer a detailed look at the impact of different kinds of party images on the vote. He finds that positive images are more correlated with the vote choice than negative images, and that in recent presidential elections the substantive content of the images has also affected the vote. References to groups and domestic policy in the party master codes tend to be biased toward the Democratic party, while references to people, government management, foreign policy, and general philosophy have favored the Republicans. The remainder of the book attempts to explain the declining correlation between the vote and party images, and why the party bias of the images held by different groups has changed since the 1950s. Regional and racial groups, social class, and education are interweaved with age and cohort variables to explain the changes. As a capstone, Trilling takes up the question of causation. Are changing party images a harbinger of a shift in party identification? or do images merely follow the change in partisanship? It is unclear; but some of the data point to images as a causal mechanism.

Popular images of the parties is an interesting topic. It is probably fair to say that our appreciation of the place of the Democrats and Republicans in the hearts of Americans is limited to comparing the proportion of identifiers to independents, and strong identifiers to weak identifiers. Occasionally we have embellished on this sketch by asking respondents a few questions about whether the parties are good or bad, useful, or important in elections.

There have also been studies of party and candidate images as they relate to the vote, but only the early Survey Research Center studies attempted to picture the popular images of the parties themselves. Unfortunately, this volume makes a negligible contribution to the lacunae.

The major problem with the book is that it is little more than a collection of tables, in which about half are initial relationships, another quarter extensions of the initial relationships, and the final quarter redundant or superfluous. Trilling's analysis of generational differences in party images is a good example. He proposes to explain shifts in party images in terms of generational replacement, yet he persists in examining image differences by age categories for each year when all he needs is the cohort measure. Perhaps there was a point to it all, but it is not clear, and the reader can be forgiven for feeling caught in an infinite loop in which much travel produces few changes in scenery. When the data are cumulative, the prose does little more than describe the data; and the problem being uncovered is lost in row by column descriptions of the tables. The turgid prose makes the effort to follow these descriptions painful.

A second and pervasive shortcoming is the author's failure to elaborate upon initial findings. Chapter 3, for example, contradicts pundits and political scientists who have fostered the idea that voters are disaffected and increasingly likely to choose the least offensive candidates. His data indicate that what voters like about the parties—and not what they dislike—continues to guide them. This finding should have been examined more closely. If it is valid, the chapter would have been much richer had it contained an examination of the applicability of the general finding for subgroups of the population. Intuitively one can think of some groups for which his overall finding might not hold. The Republican tilt of the South, for example, might be based more on negative images of the Democrats than on positive images of the Republicans. And the South may be typical of the many segments of the electorate. Even if it is not—and only a few groups deviate from the general pattern—we would have better models of the electorate if we knew for certain that southern whites, blacks, urban Catholics, and organized workers were responding to the parties along positive lines rather than along their distaste for the alternatives.

Finally, Trilling's attempt to use generational replacement to explain changes in party images is very unsatisfying. Class, education, age, and cohort comparisons are the stuff of the analysis. Why educational and class groups are the central focus of the analysis is never clearly spelled out. And, as before, the analysis doesn't extend far beyond a description of the contents of the tables. What is clear, though, is that the analysis of transformations in party images is supposed to climax around the role of generational replacement. So, what happens? On p. 116 Trilling writes that the "effects of generational replacement are obvious." Subsequent data cast doubt on that, if they were clear the first time, so p. 123, "reaffirms" the importance of the "changing age and educational composition . . . of the electorate . . . for the declining salience of class in the party images of Americans." His confusion can be forgiven. The class and education analysis of party images is done so many ways within age and cohort breakdowns, that it is difficult indeed to spot a trend. As I read the data, there appear to be few generational differences. Much of the change appears to be election-specific; and what seem to be cohort differences are often either perturbations or a secular trend affecting all age groups.

The concerns which animate this book are worth the attention of students of electoral behavior. Unfortunately, this analysis is inelegant and incomplete. With a few exceptions (Trilling's data on southern party images), this book only calls attention to an area which needs research.

JOHN PETROCIK

University of California, Los Angeles

Small Town Police and the Supreme Court: Hearing the Word. By Stephen L. Wasby. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xi + 264. $18.00.)

Wasby is right when he says that we know very little about how implementing organizations get information about court decisions. The most useful study would focus on these organizations and explain how information is supplied to them. It would say something about the obstacles to adoption of new policies based on this information. How important is better communications as a catalyst for change? Answering the latter question requires a perspective that thoughtfully considers the relationship between communications and its context. Wasby speaks to all of these issues in his study of small-town police departments, but his contribution is limited because despite its promise the study is not well informed by useful approaches. It also lacks a perspective sufficiently sensitive to the political context of information flow. It is not that he is unaware of these issues; he simply gives them insufficient attention. Thus, the book adds little to existing knowledge and reflects a rather troubling perspective on the role of law.

Wasby could have avoided some problems if he had in fact taken the diffusion-of-innovation approach that he promises to follow (p. 1). Diffusion theory is well grounded in empirical research, much of which applies to manifestly political issues. The approach is parsimonious in its identification of the key variables. But Wasby uses it only as the basis for a fragmentary post hoc analysis (pp. 223 ff.). He offers no information about the speed of innovation. His uncertain focus exacerbates this problem. (In both Massachusetts and Illinois he interviewed 25–30 police chiefs or sheriffs. He also talked to state police officials, local judges, prosecutors, and public defenders. One is never sure where the information best applies.) Is the focus on the individual police officer? If so, why not relate diffusion to individual characteristics? Is the focus on departments? Then why not use organizational characteristics as independent variables? The overly long and sometimes inchoate presentation of the findings and literature review (for example, Chs. 2, 5, p. 66) is related to this lack of focus.

Wasby also pays insufficient attention to the content of the information. Despite the fact that the police officials claim that training information is important, there is virtually no information as to what training bulletins or similar communications actually say.

There are problems even if we consider what Wasby does rather than what he promises. Interviews form almost the entire basis for the investigation into how information is transmitted. Such data are more limited than Wasby acknowledges. It is not simply the absence of "posturing" (p. 220) that is important. The real issue is what interviews alone can tell us about the process involved in receiving and digesting information. One cannot depend on them to discover how officers interact with one another in training sessions or what prosecutors tell chiefs over coffee. We need additional behavior measures based on observation.

This leads Wasby to tentative conclusions, e.g., that Massachusetts police officials discuss legal issues more frequently with prosecutors "perhaps as a function of the greater frequency of informal contacts" (p. 132). Maybe so, but on his evidence I am less willing to accept this conclusion even when put so tentatively. It would have been far better to look at such questions in more detail than to get sketchy information from many departments.

The communications approach can be useful, but it is all too easy to overemphasize the importance of information. What seem to be communication gaps are often severe conflicts of interest. Wasby succumbs to this overemphasis. In his concern with communication gaps he pays insufficient policy attention to such conflicts and to factors which inherently limit clarity of information.

In fact, the achievement of clear information about court decisions has significant limits. In Wasby's opinion, the Burger Court may be cueing the police to pay less attention to the law. On the contrary, the police may pay *more* attention now that decisions may have become more sympathetic to their needs. Reformers who in the days of the Warren Court used court decisions as benchmarks for police behavior can no longer do so. They face the problem of getting the police to behave with more restraint than they would if they followed the letter of the law. They will have to encourage police discretion and alternative systems and norms. In effect, not the Burger Court but its opponents must encourage alternatives to a reliance on the rule of law. Otherwise, one might unduly rely on the rule of law as Wasby does when he concludes:

> Yet if the police were to begin to take their cues regularly from the courts . . . that by itself would without question bring us closer to the *desired* norm of a rule of law, *whatever its content* (p. 229, my emphasis).

Do we really want to accept the rule of law regardless of its contents?

NEAL MILNER

University of Hawaii

Managing the State. By Martha Wagner Weinberg. (Cambridge, Mass.: The MIT Press, 1977. Pp. x + 244. $15.00, cloth; $5.95, paper.)

This book is a study of the problems of managing the state from the perspective of the governor. Its primary conclusion is that "the most important characteristic of elected chief executives, if the Massachusetts case is representative, is their reliance on crisis management" (p. 208). "Governors do not manage in the customary sense of the word, that is, they do not direct or oversee the affairs of agencies over some sustained period of time. Instead, management for elected chief executives usually involves sporadic intervention in the agencies' business, often only for a short period of time. This intervention is most often initiated because of a crisis" (p. 209).

The theoretical framework of the study rests on the proposition that decision making by a political executive cannot be exaplined satisfactorily either by the rational decision-making or the muddling-through model. While these models are helpful in certain situations, it is only the crisis management model that helps to explain the seeming inconsistencies in the chief executive's behavior which are "not predicted by either the rational actor model or the muddling-through model" (p. 210).

For example, the rational decision-making model does not take into account the fact that elected chief executives seldom have time to calculate long-range strategy. They must act rapidly on the basis of incomplete information. Furthermore, since the agencies which governors are attempting to manage are public, they cannot totally control them. In addition, they have neither the time nor the resources to manage a problem from its inception to its conclusion. Their intervention is on a spasmodic crisis basis in which they insert themselves into the policy process of any given agency perhaps only once or twice or even not at all during an entire administration.

Martha Weinberg finds that the muddling-through model of decision making is deficient in part because of its emphasis on incrementalism. She points out that the governor does not knowingly sacrifice primary values, such as the maintenance of public support, to the dictates of incrementalism. In addition, she maintains that a governor's input may have not simply a marginal effect, but a decisive one, on decision making. As a persuasive example of her position, she points out that in Massachusetts "a highway network was stopped because of Sargent's decision" (p. 211).

The author also argues that: "Governors do not sit in their offices and explicitly lay out their goals, objectives, and plans in accordance with the rank order of their priorities. Nor do they muddle through their daily management duties by making incremental changes in policies in order to move gradually toward different goals. Instead they attempt to respond to the publicly generated or personally perceived stimulus of a crisis" (p. 218).

These discussions of the weaknesses of the rational and muddling-through models when they are applied to the elected executive decision-making process give this book a potentially wider audience than would otherwise be the case. "Decision making" is, for the moment, the magic phrase in political science. Consequently, those who study that science (art?) should consider Weinberg's development of "crisis management" as a contribution to decision-making theory.

Aside from its testing of decision-making hypotheses, this book also analyzes the functions of the governor. Here it presents some difficult problems. While we may accept the argument that governors are concerned from time to time with management of crises, it is quite another matter to say that this is the chief characteristic of the gubernatorial function.

The conclusion that crisis management is the governor's principal function is not entirely clear from the evidence presented. In chapter 3 the author herself introduces substantial evidence to show that management, crisis or otherwise, was not a primary concern of Governor Sargent, who, as she notes, did not enjoy management and actually devoted very little time to it. She also points out that he devoted little time to the legislature; his posture was that since the legislature was heavily Democratic, it failed to follow his leadership for partisan reasons and that he was a lone Republican governor fighting the good fight. He did spend more than half his time on his "ceremonial functions" including many public appearances. Thus, if it is Weinberg's point that management of crisis is the governor's principal function, there is some doubt that her evidence supports her conclusion. On the other hand, if her point is that crisis management is really the way the governor goes about one of his functions, i.e., "managing the state," then the evidence is more persuasive.

Perhaps my main reservation regarding this book is expressed in the quotation cited above: "The most important characteristic of elected chief executives, *if the Massachusetts case is representative*, is their reliance on crisis management" (p. 208, italics added). Since the author's conclusions are based on case studies of the relationship of one governor—Sargent—with four of his agencies during one administration, we cannot be sure that the conclusions drawn are typical either of Massachusetts under other governors or of the governor's role in other states.

COLEMAN B. RANSONE, JR.

University of Alabama

Comparative Politics

Politicians and Soldiers in Ghana 1966–1972. Edited by Dennis Austin and Robin Luckham. (London: Frank Cass, distributed by ISBS, Forest Grove, Oregon, 1976. Pp. xiii + 318. $27.50, cloth; $9.95, paper.)

Twelve experts on Ghana politics, four of whom have authored monographs on related topics, contributed twelve varied pieces on the years 1968–1972 for this collaborative effort. The period chosen, with its transfer of power from a military to a civilian regime and subsequent return to military rule, provides fertile ground for analysis of some fundamental political issues. The team approach to the study has produced some good results. The division of labor among authors produced a thoroughness and balance beyond what could be expected in a single monograph. Most notably, it permitted a look at both national and local politics, with four chapters devoted to electoral politics in selected constituencies. Moreover, the chapters cross-reference each other, making the collection appear cohesive.

Unfortunately, the book fails to live up to the promise of its title. Most obviously, it focuses almost entirely on the preparations for and conduct of the 1969 general elections, which signaled Ghana's return to civilian rule and brought K. A. Busia's Progress Party to power. Events after 1969 are discussed only briefly in editor Austin's introduction, John Essek's chapter on economic policies, and Valerie Plave Bennett's epilogue.

But even a study of the 1969 elections should have given readers a greater understanding of the relationship between civilian and military regimes than this work does. The approach to the election can best be described as belonging to the "who won and why?" school of analysis. Most of the authors seem to regard the Progress Party's victory as both inevitable and an auspicious sign for Ghana's political future, although John Dunn surpasses the enthusiasm of the others when he concludes in his chapter on the Progress Party's victory in one division: "The story of how the electors in Asunafo . . . chose virtue is a story which deserves a share of honor even outside its own country. What else better, in all innocence, could they in fact have done" (p. 208).

More importantly, the "who won and why?" approach ignores some very important questions about the electoral process in the new states of the Third World. Why focus on who wins elections when the results are often predetermined and when the winners' prospects for remaining in office to the next election are scarcely better than even? What we need to know in a book about "politicians and soldiers" is whether the competitive electoral process contributed to or detracted from the stability of the newly formed civilian regime. It would not be unreasonable to argue that the popular election of Busia's government should have provided it with a reservoir of support and legitimacy that would enable it to survive the bad times of economic austerity. Such logic conforms to democratic ideology, and it is reasonable to assume that military leaders, despite their monopoly of force, are reluctant to overthrow popular regimes. On the other hand, Huntington has argued persuasively that competitive elections in new states often diminish the chances for survival of civilian regimes, because they tend to aggravate latent tensions in the political system. Emily Card and Barbara Callaway in a short 1970 article in *Africa Report* suggested that the tensions aroused by the 1969 elections, particularly those related to Ewe-Akan rivalries, did not bode well for the future of civilian politics in Ghana. In light of the 1972 coup, their analysis is both more prophetic and more suggestive theoretically than this book, most of which was also written before the coup despite its 1975 publication date.

Despite the above misgivings about the questions dealt with by *Politicians and Soldiers*, it does contain considerable information on the politics of Ghana in the late 1960s. It belongs on the shelves of those interested in the politics of this important West African state generally, the efforts of military rulers to remove themselves from overtly political roles, or elections in new states.

JAY E. HAKES

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Japan: The Paradox of Progress. Edited by Lewis Austin. (New Haven and London: Yale University Press, 1976. Pp. vi + 338. $20.00.)

The editor's final note in this collection of essays is that "the primary paradox of progress is that it continually outmodes itself" (p. 302). This very paradox contributes to the rapid dating of some, but not all, of the essays. The reviewer's inclination is to compare each of the 11 essays to the others—all of which were presented at the 1973 Seminar on the Future of

Japan at Yale. Instead, I will focus only on those essays which have either a direct or an indirect impact on the understanding of politics in Japan.

Four essays are clearly political (listed as short titles): "Elections, Coalitions, and Prime Ministers" (Nathaniel Thayer); "Urban Local Politics" (Terry MacDougall); "Problems for Higher Education" (William Cummings); and "The Political Culture of Two Generations" (Lewis Austin). The remaining essays include: "Japan in World Economic Enviromments" (Hugh Patrick); "Technology and Power" (Dixon Long); "Impact of Changing Labor Force Characteristics" (Robert Cole); "Organizational Determinants in the Factory" (Koya Azumi and Charles McMillan); "Japanese Spirit in the 1980s" (Robert Frager and Thomas Rohlen); "*Danchi Zoku* and the Metropolitan Mind" (Christie Keeler); and "The Evolving Views of Women" (Susan Pharr).

Thayer's valiant attempt to predict elections and coalition behavior more than ten years in advance was risky. His prediction did not even make it through the 1976 election, wherein he predicted success for the ruling Liberal Democratic Party at 44.6 percent of the votes, which compares to an actual 41.8 percent. Two unforeseen intervening variables were the Lockheed scandals and the defection of the New Liberal Club from the LDP. Both cut into the vote, changing not only this election but altering the future calculus on both elections and coalitions.

MacDougall engages in less predicting and more explaining. Out of a rich data base he derives the hypothesis that "the effectiveness of progressives in urban local politics" is the result of the advent of progressive parties, citizen discontent, and the "singularity of the mayoral or gubernatorial posts" (p. 53). Ample support is given to show the predicted causal relationships.

MacDougall's attempt to show that it might be possible to transfer progressive success in local elections to national elections is less well done. It is true that the Socialists have turned to the most popular and successful of the progressive mayors as their new party chairman, but, unfortunately, this was because of the party's failure in the last elections (plus the inability of the leaders to hold the party together). The electoral failure, wherein the Socialists gained only five seats (while the Communists were losing 21 seats), negates the proposal that local machines and local cooperation can lead to national victories. The weakness could be that MacDougall has not addressed the fact that building a progressive coalition for the single office of mayor (a zero-sum situation) is quite unlike doing the same with a multi-member, single-vote national district (non-zero sum).

Cummings analyzes the political forces influencing five issues of higher education—goals, control, scale, quality, and diversification. The underlying question is universal: Who governs? Three new laws emerged from the authority conflict—each strengthening the central role, but not to an extreme. The democratic problem-solving process itself contributed to defusing the high conflict; i.e., election politics forced the moderation of the ruling LDP on the right and the realities of economic and political life forced the moderation of the radical parties and groups on the left.

Austin's essay, although more jargonistic than the others, is strongly scientific. His hypothesis on generational change is that "industrialization and social differentiation imply political-cultural convergence in the direction of liberal, secular, and progressive values" (p. 233). He rejects the hypothesis because of the complexity of the variables, leaving the political world still uncertain, "but much more interesting, too" (p. 254).

As for the essays indirectly related to politics, the two longest (50 pages each) focus on economic conditions. Patrick's concern is with Japan's growth performance and international relations in the 1980s. His analysis focuses on three hypothesized scenarios—"cooperation," "conflict," and "competition-for-resources." Cole's key question is: What are the causes and effects of the changing labor force?

Keifer's primary concerns are the socio-psychological impacts of life in dense, high-rise public housing (*danchi*). The secondary political consequences include provision of transportation and educational facilities and voting behavior. Political leadership emerges differently when a community appears overnight, somewhat like Athena springing full-grown from the eye of Zeus. Keifer hypothesizes that the psychological problem of loss of identity and personal power are related to the individual's surrender to a powerful mass identity (state, religion, or corporation). Kiefer rejects this as unlikely for Japan, where the individual seeks identity in traditional, tight-knit, face-to-face political movements.

Pharr's essay on changing roles of women also has political ramifications. The key variables of change are: (1) less need to be at home; (2) more need to be employed; and (3) higher education and higher expectations. The three resultant types are: "Neotraditionalists," "New Women," and "Radical Egalitarians." The immediate future, she concludes, probably belongs to the "New Women."

Austin has skillfully summarized and related the essays in his introductions to the book and each chapter. Overall, this is an excellent and important collection, even though some of the "paradoxes" were a bit strained. It makes a great title anyway.

LEE W. FARNSWORTH

Brigham Young University

Détente and the Democratic Movement in the USSR. By Frederick C. Barghoorn. (New York: Free Press, 1976. Pp. x + 229. $12.95.)

Both détente and the democratic movement in the USSR are by now familiar subjects. Détente has been the issue in an extensive American foreign policy debate. The leading Soviet dissidents have become virtual household words, and subjects of a growing scholarly literature as well. Still, there is much more to be said on a number of topics to which this book devotes attention—philosophical disagreements among the most well-known dissidents, the content of dissent with which we are less familiar, the interconnection between détente and dissent. Frederick Barghoorn, who has previously written on both Soviet foreign policy and dissent, is well qualified to tie the two together, and to a substantial degree he has succeeded in doing so.

Barghoorn's book has a two-part thesis—"that in the Soviet system as it is presently constituted, there cannot be any significant toleration of dissent, and that this intolerance is a powerful indicator of the limited, one-sided character of détente as practiced by the Kremlin" (p. 126). Such propositions, hinging as they do on subjective definitions of "significant" and "one-sided," are in a sense non-falsifiable. Readers with a preference for formal hypothesis testing may find this frustrating, but Sovietologists are used to operating more impressionistically. Barghoorn notes with satisfaction that his major proposition is "surely difficult to disprove" (p. ix). His "proof" consists largely of a series of summaries of dissident views, plus a choice selection of incriminating quotes from Soviet leaders, plus the author's interpretation of both of the above.

Barghoorn is at his best in analyzing the trialogue among the Big Three of Soviet dissent—Aleksandr Solzhenitsyn, the neo-Slavophile, Andrei Sakharov, the liberal westernizer, and Roy Medvedev, with his hope of recapturing genuine Marxism-Leninism. Excellent also is the book's treatment of dissident tendencies less well known in the West, for example the "New Left" of the 1960s (particularly the radical internationalism of Yuri Galanskov), and various currents of unorthodox nationalism like that of Ukrainian dissident Valentin Moroz. For the most part, Barghoorn lets the dissenters speak for themselves, but when he does break in it is usually with an observation of insight and importance. Solzhenitsyn, who calls the West into battle against Soviet tyranny even as he warns of western weakness, is, as Barghoorn notes, simultaneously pro-western and anti-western. Particularly instructive are passages in which Barghoorn reaches beyond Sovietology for political science concepts which illuminate his theme. Thus, for instance, his use of Robert Dahl's notion of "hegemonic regime" to explain "the persistence in the USSR of a cyclical, or vicious-circle pattern of repression-relaxation-dissent, followed by renewed repression" (p. 123).

One problem with this book flows from its reliance on summaries of dissident thought, author by author, work by work. As Barghoorn himself notes, a number of these writings "defy summary," yet summarize he is determined to do. The device is helpful in the case of works not yet translated or widely available in English, but it seems unnecessary when a book or article is familiar to the reader. One wishes that Barghoorn had spoken out more often in his own distinctive and distinguished voice.

The fact that the book was originally written as an unclassified study for the United States State Department's Office of External Research may account in part for the author's penchant for policy prescription. In any event, he concludes that "the record examined in these pages should help to alert us to the threats to American national interests and to democratic values around the world posed by . . . Soviet foreign policy. . ." (p. 146). But instead of laying out the full case concerning this threat, Barghoorn is too often tempted into easy polemical sallies against the Soviet regime (with its "vengeful, despotic Kremlin spirit"), and against what he perceives as the naivete and sheer incompetence of recent American policy, for example the "amazing ineptitude" of President Ford in not receiving Solzhenitsyn at the White House (pp. 131, 87).

Some of the space devoted to summaries and sallies could have been better employed exploring key substantive issues. According to Barghoorn, "It would be difficult to imagine anything more salutary for world peace and welfare than the 'democratization' of the USSR" (p. 171). Yet though he returns to this theme several times, Barghoorn never digs deeply into the issue, particularly into the prospect posed by Andrei Amalrik that in a country lacking a democratic tradition and with a myriad of clashing nationalist animosities, democratization could produce disintegration and anarchy. Similarly, there is the issue of just how widespread is support for the dissidents—which bears on the further question of how much of a "movement" the "democratic movement" really is. At several points, Barghoorn notes the dissident claim that, as Sakharov, Medvedev and Valentin Turchin put it in a 1970 manifesto, the "preponderant part" of Soviet "intelligentsia and youth" understand the need for democracy. Yet in the end (p. 165) the author rejects that claim without fully elaborating the grounds for his skepticism.

Readers who have followed neither the debate about détente nor the growing literature on the democratic movement, and those who are acquainted with one but not the other, will find this book a helpful introduction. Readers who have kept track of both developments will glean occasional striking insights and useful references.

William Taubman

Amherst College

The Politics of Cultural Nationalism in South India. By Marguerite Ross Barnett. (Princeton, N.J.: Princeton University Press, 1976. Pp. xii + 368. $22.50.)

The existence of linguistic, religious, tribal, and ethnic identities in most of the developing nations has given rise to conflicting theories about their significance for political integration and development. Urban nationalist elites of the developing countries have condemned movements based on primordial identities as threats to national unity and to economic and social development. The fears of nationalist elites have often been reinforced by social scientists who have characterized primordial ties as obsolete or merely transitional forms of traditional identities which inevitably must give way to the civil ties of the modern nation state.

Marguerite Ross Barnett joins a growing roster of scholars who have challenged this assimilationist view of political identity. Barnett rejects the notion that primordial ties must be static expressions of unchanging relationships. Political identity, she argues, is always created. It is malleable, shaped by leaderships and public policy, and, thus, subject to change which may be analyzed. The major questions, therefore, in the study of political identity are: What forces shape political identity, how do they become transformed and redefined, and how do they become politicized?

In order to demonstrate her thesis, Barnett analyzes the social, political and economic factors that give rise to a cultural nationalism that became expressed through the Dravida Munnetra Kazhagam (DMK), a South Indian political party which has successfully used cultural nationalism for purposes of political mobilization. Unlike many contemporary studies of change which attempt to analyze intervals of relatively short duration, Barnett traces the development of political identity in Tamil Nadu over a period of 60 years. The result is a clear demonstration of the malleability of political identity as it developed in South India. Studies limited to segments of her continuum would have revealed quite different and conflicting notions of the "primordial," from which all further development would then have been presumed to issue.

The development of cultural nationalism in South India, Barnett argues, was not simply the result of objective conditions. Cultural nationalism had to be defined so as to elicit a collective response before it could be used for political purposes. The first step was the emergence in the 1920s of the concept "non-Brahmin," a cultural construct which encompassed diverse castes within a single political identity. By 1938 non-Brahminism gave way to a more distinctively Dravidian ideology which demanded a separate Dravida Nadu or Dravidian state. By 1949, however, Dravidian separatism has been supplanted by a Tamil cultural chauvinism which retained the now familiar symbols of Dravidian resurgence. Thus, cultural nationalism defined in different ways at different times served the DMK and its predecessors as a mechanism for political mobilization.

This valuable study of cultural nationalism is also one of the most comprehensive studies of the DMK yet written. It is based on archival material, vernacular pamphlets, 200 elite interviews and a sample survey of 700 local-level leaders. It covers the history of the Justice party from which the DMK evolved, the elite and mass base of the present party, its sources of internal tensions, and the role of the DMK both as opposition and as government.

The author draws several important political lessons from the Indian experience, but she fully acknowledges that these lessons should be extended to other societies with caution. In the first place, cultural nationalism need not be incompatible with territorial nationalism, but may add a new dimension to it. Second, institutional mechanisms such as federalism and decentralization may play an important role in mitigating conflicts which emerge between cultural nationalism and territorial nationalism. Finally, cultural nationalism is not necessarily non-rational so much as an expression of important underlying interests which, even though they may have ultimately proved to be noncompatible with one another, may provide an important basis for mobilization. Overall, then, contrary to expectations, cultural nationalism proved to be a positive force for significant social change.

Barnett's caution in extending the lessons of her study to other societies is well taken. It may be that the markedly pluralist nature of Indian society has made possible the type of accommodations she describes. Unfortunately, there is no discussion of what this pattern of accommodation costs in terms of economic growth and development. Finally, can the zero-sum nature of cultural nationalism be totally dismissed as solely the obsession of nationalist elites?

The book is a model of political science research, a magnificent example of how modern political science methodology and area studies can be blended to produce significant results. The book deserves to be read not only by area specialists but by all political scientists interested in the problems of political development, political parties, ideology, and political behavior.

STANLEY A. KOCHANEK

Pennsylvania State University

Forces of Order: Police Behavior in Japan and the United States. By David H. Bayley. (Berkeley: University of California Press, 1976. Pp. xvi + 201. $10.95.)

Bayley's study of Japanese law enforcement is an outstanding contribution to the police literature and to the growing number of studies in comparative criminal policy. Accompanied by an interpreter, the author spent two summers observing police operations in various urban and rural settings throughout Japan. His inquiries met with cordial, at times enthusiastic, reception—a circumstance which can only be regarded with envy by students of police and by cross-cultural researchers in general. Bayley makes no effort to conceal either his gratitude or his admiration for his hosts.

The book's initial chapter discusses the rates of crime and police misconduct in Japan, both of which are astonishingly low by the standards of most western industrial societies. The contrast is even more stark given Japan's degree of urbanization and industrialization, its tradition of violence, and its relatively recent history of aggressive imperial expansion.

After an excellent discussion of police-community relations, subsequent chapters explain this apparent anomaly. Despite its organization as a national police, the Japanese force is considerably decentralized. Personnel of the fixed police posts, called *koban* and *chuzaisho*, serve urban neighborhoods and rural villages, respectively. Police-citizen contacts thus tend to be more personal and less mediated by machinery than in those police systems which rely primarily on motorized patrols. The role of the police in the community is also distinctive, and reflects a strong service orientation. Japanese officers routinely provide counseling services, and Bayley relates how officers nearing the end of a 24-hour shift listen with patience and sympathy to the occasional derelict who seeks a little companionship.

The author characterizes the Japanese style of policing as self-effacing, low-key, and undramatic, and suggests at one point that Japanese officers are as "inconspicuous as postmen." The specialized riot police, highly trained and well equipped, constitute a departure from this characterization. But even these *Kidotai* show relative restraint in response to riots and protest demonstrations and conduct most of their operations without recourse to firearms. As might be expected from this controlled demeanor, hostility between police and public is limited to the antagonism which prevails between members of the force and youthful dissidents. Policemen, most of whom are recruited from rural areas of Japan, harbor a strong antileft bias, and their lack of sympathy is reciprocated by many students. In this respect, police-community relations correspond to an apparent worldwide pattern.

Other subjects treated in the course of the book include the worklife of the policeman, disciplinary procedures, the control of victimless crimes, and the importance of informal sanctions and contrition in Japanese society.

If there exists one shortcoming in Bayley's book, it is his failure to discuss the extent to which police in Japan engage in such covert activity as the infiltration of dissident political groups. The omission is all the more regrettable, given the abuse of such activity by many police forces, with the attending adverse impact on the public image of the police. In this regard, the chronic hostility between the police and student dissidents in Japan leaves a good deal to the imagination.

The author's analytic approach consists of discussing a particular phenomenon in its Japanese context, embellishing it with illustrative anecdotes drawn from his own observation or from the recent past, then briefly contrasting it with a comparable aspect of policing in the

United States. The reader's understanding of Japanese law enforcement, and of police behavior in the U.S., becomes that much richer. Both anecdotal and comparative aspects of the work are nicely integrated. The book reads superbly, and the reader does not get the impression of being bounced back and forth across the Pacific. Typographical errors are few in number, and aside from one reference to a "complaint population" (p. 147), are not troublesome.

What lessons, then, may be learned from the Japanese police? A number of their institutions and practices, such as a lengthened training program and the neighborhood police post, could be transplanted with relatively little difficulty. Others, such as semiannual residential surveys, required registration of all weaponry, and the total prohibition of privately held handguns, would meet with fierce resistance on the part of the American public. And if, as Bayley contends, police institutions are shaped by social context, there is little that can be done in an intensely individualistic society to reproduce the social homogeneity, deference to authority, the salience of groups in the development of personal identity, and the sense of civic duty which greatly simplify the tasks of police in Japan.

In brief, Bayley's book is a model study. It richly deserves replication elsewhere, assuming that the requisite degree of research access is attainable. Meanwhile, *Forces of Order* merits not only the attention of a wide interdisciplinary audience, but the consideration of police and policy makers as well. Many police systems throughout the world can borrow from the Japanese; all can learn from them.

P. N. GRABOSKY

University of Vermont

Paradoxes du pouvoir local. By Jeanne Becquart-Leclercq. (Paris: Presses de la Fondation Nationale des Sciences Politiques, 1976. Pp. xii + 233. 115 F, cloth; 80 F, paper.)

Development and Social Change in Yugoslavia: Crises and Perspectives of Building a Nation. By Peter Jambrek. (Lexington, Mass.: Lexington Books, D.C. Heath and Farnborough, England: Saxon House, 1975. Pp. vii + 280. £6.95, $19.50.)

The City in Comparative Perspective: Cross-National Research and New Directions in Theory. Edited by John Walton and Louis H. Masotti. (New York: Wiley, Sage, 1976. Pp. vii + 317. $17.50.)

Just a decade ago many European specialists maintained that local politics was insignificant, that only local administration mattered. Doubtless local autonomy varies, and U.S.-based analyses can be sharpened by attention to patterns of intergovernmental relations abroad. More reasonable than arguing that local autonomy does or does not exist in a given country is to focus on its variations and correlates. Most of the works reviewed here move in this direction.

The British were the earliest to develop a literature on local politics, which is actively continued in the journal *Policy and Politics* and elsewhere. The French have always been perhaps the most resistant to the study of local politics. Crozier et al. maintain a sophisticated approach to local administration, and several French Marxists have used examples from local politics to support their theories (see the exchange in *Revue francaise de sociologie*, 15 [1974]), but Jeanne Becquart-Leclercq has published the first serious study of local politics in France. She interviewed mayors and other informants in 41 communes in northern France, showing how they are recruited to office and distinguishing the types of roles they play. A chapter on participation documents the enormously high (near 90 percent) turnout in local elections in small towns. This seems explicable by noting similar pressures found in well-organized precincts in Chicago: local political leaders know who does and does not vote. This type of political organization has been largely ignored by recent theorists of nonvoting; it deserves more attention.

Particularly fascinating is Becquart-Leclercq's documentation of linkages between Paris and these communes. They are compared on a basic policy output—a performance index of the level of local public works (such as having electricity, sidewalks, etc.) Twenty-three explanatory variables were considered, and a multiple regression shows that the mayor's informal linkages with Paris (pp. 155–57, 221–32) were second in importance. These linkages were ingeniously operationalized using items including importance and frequency of social contacts with local and national officials. Cities where the mayor was well connected with Paris got more public works. This powerful finding demands replication in other national contexts. A partial translation into English may be found in *Power, Paradigms, and Community Research*, ed. R. Liebert and H. Immerschein (Sage, 1977).

Jambrek is interested in local politics as related to economic development. He criticizes Lipset and others who have suggested that socialism and democracy are incompatible, but he does not go as far as Schumpeter in denying any necessary relationship between the two. Rather he sketches a developmental approach suggesting that democracy is unlikely in the early stages of socialism, but possible as economic development proceeds.

The empirical analysis investigates this general idea and many more specific propositions about intergovernmental relations, the role of the League of Communists, and variations in centralization of decision making. Data derive from a comparative study of 16 communes and case studies in two by the author as well as summaries of numerous surveys, content analyses and field studies completed by associated Yugoslav researchers.

Among other findings, he reports the opposite patterns from Becquart-Leclercq. That is, communes with the most intergovernmental ties are the least developed. But unlike the French case, this seems to derive from an explicit policy of sending more assistance and personnel from regional agencies to the least developed communes. The resulting volume is general enough to constitute a useful overview, but specialized enough to be significantly original to help researchers on local politics in any country.

The Masotti and Walton volume derives largely from the September 1975 *Urban Affairs Quarterly.* The papers are uneven in quality and many barely touch on politics. The two editors provide a useful overview and discussion of methodological problems of cross-national urban research. Janet Abu-Lughod similarly discusses related themes with Egyptian examples.

Philip Jacob reanalyzes the data from the International Studies of Values in Politics for Polish, Yugoslav, Indian, and American local leaders, focusing on local autonomy. He finds an interesting paradox: the correlates of local structural autonomy (what the city can or does do) scarcely help to explain the preferences of local leaders regarding autonomy. Jacob sensitively discusses a number of alternative interpretations.

Michael Aiken compares local party competition by assembling census, voting, and related data for about 150 cities in each of four countries: France, Belgium, Italy, and the Netherlands. Like Jambrek, he finds that more social and economic differentiation leads to more competition. Again like Jambrek, he finds a positive relationship between competition and socioeconomic status of the residents.

Concepts like autonomy, competition, and participation obviously change in meaning in these different national contexts. But examination and replication of relationships, in quite different national contexts such as these, are important steps toward a generalizable theory of urban politics.

TERRY NICHOLS CLARK

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Values and Community in Multi-National Yugoslavia. By Gary K. Bertsch. (Boulder: East European Quarterly, distributed by Columbia University Press, 1976. Pp. xiv + 160. $10.00.)

Surveying 1,186 Yugoslavs to discover how successful Yugoslavia has been in building the "community of peoples" its constitutions have claimed for it, Gary Bertsch prompts us to consider two things: how a state whose ideology claims to have done away with nationalism copes with it, and whether culturally diverse peoples can live cooperatively within a federal political system. These questions are permanently interesting for the student of nationalism, permanently troubling for political leaders who must deal with it.

Bertsch begins and ends with the conventional assumptions about social cooperation: high levels of economic development produce high levels of social mobilization, universal rather than particularistic values, and the communication, cooperation and understanding which foster community. Most of the book is devoted to exploring variations on this theme. Bertsch ranges from the predictable, however, to ask some interesting questions about the effects of style and culture—what in referring to the south, for instance, the Yugoslavs are prone to characterize as "500 years of Turkish oppression"—on communitarian values or their absence. The book is worth reading for its insights into the behavior of the Slovenes who, in all measures of "modernity" outdistance their compatriots but who are nonetheless exceptionally particularistic and intolerant toward other Yugoslav nationalities. Despite these anomalous values, however, the Slovenes embody Mayor Daley's advice: they "don't make no waves, don't back no losers." In contrast, the Croats, who do make nationalist waves, according to Bertsch's findings are less particularistic and more supportive of universal values. Apart from drawing the conclusion that opinions and behavior are not necessarily related (an issue Bertsch takes up only briefly, in distinguishing between elite and popular attitudes), one might well consider the Slovene case before applying too readily the traditional measures of political cohesiveness. Slovenia suggests that integrative values are neither necessarily as "modern" nor as important as we make them out to be.

Precisely because Bertsch points out the imperfect fit between universal values and political integration, it is discouraging for the reader who hopes to learn something from this book to find the following sentence: "Yugoslavia's attempts over the long run to build a modern, integrated political community are likely to be successful to the extent that all sectors within the society develop value systems engendering such a political form." It is fortunate for the author that this ghost of *The Civic Culture* appears on page 104, for such a tautology offered not in conclusion but as a hypothesis should deter any reader.

Equally troubling, given Bertsch's repeated expressions of interest in individuals and individual values, is the paucity of information on individuals. A few intriguing personal portraits appear early in the book, but while he tells us that it was long conversations which persuaded him that communitarian values are indeed growing in Yugoslavia, Bertsch does not let us in on those conversations. Instead, the data is all aggregated and depersonalized. Particularly, inasmuch as he suggests weighing the effects of cases of individual isolation against the blanket presumptions commonly associated with "cultural heritage," his work sorely needs the evidence of more life histories. Bertsch must have recorded those conversations, but instead of using them he falls back on familiar but inadequate methodologies. There are too many tables like the one proving an inverse relation between superstition and education. The social science code enjoins us from lying with statistics; it should also insist that we abjure them when English will do.

CYNTHIA W. FREY

The National Endowment for the Humanities

Democracy and Organisation in the Chinese Industrial Enterprise, 1948–1953. By William Brugger. (New York and London: Cambridge University Press, 1975. Pp. 374. $27.50.)

By the year 2000, China's leaders intend to transform their nation into a modern industrialized state by means of advances in science and technology coupled with increased worker discipline and productivity. Keeping in mind these contemporary aspirations while reading this insightful account of Chinese efforts of some 30 years ago to reorganize industrial enterprises, one is struck by the continued recurrence of certain organizational problems in the Chinese modernization effort. The meticulous scholarship which buttresses the author's carefully stated observations and judgments on Chinese enterprises makes this volume important not only for the political economist or historian but also for the contemporary analyst.

Though by his own admission, Brugger started out to describe how the Chinese government took over enterprises and brought worker participation into the factory, he soon moved (at the urging of early readers of the manuscript) into the broader questions of the logic of industrialization and the historical experience in which the Chinese efforts must be viewed. These broader concerns dictate much of the volume's organization.

The volume starts with an analysis of the Chinese pattern of industrialization in the context of the nation's history. There are two important aspects to Brugger's observations: (1) a discussion of the Japanese influence on Chinese efforts in the pre-1949 period (one wishes he had devoted more attention to this aspect of the work) and (2) a discussion of the Soviet model. While all accounts of industrial development in China mention the Soviet model, Brugger is singularly useful in alerting the reader to the vagueness of the term in the Soviet context, a vagueness which had implications for applicability in the Chinese scene. In this chapter a number of comments foreshadow contemporary issues of the 1970s. For example, Brugger notes the historical tradition of the concept of "group solidarity" in Chinese experience, an observation useful to consider when many attribute "radical" policies solely to Maoist experience and ideology.

Brugger also writes at some length about the political environment in which Chinese efforts in the enterprises were set. These pages are particularly valuable for insuring that the reader recognizes the immensity of the problems confronting the post-1949 leadership together with the obvious limitations they would impose upon the specific problems analyzed in this volume. A key element here is the degree to which resource shortage and low skill level influenced policy choices and hampered some policy implementation.

The heart of the volume, however, is devoted to Brugger's analyses of the central problems of integrating democracy in the organization of China's industrial enterprises in the five-year period 1948–53 (Chs. 4–7). The three issues addressed were: (1) norm determination and the establishment of a planning and accounting system (Ch. 4), (2) rationalization proposals, production competition, incentive policies and labor agreements (Ch. 5) and (3) the establishment of responsibility and a new discrete command structure (Ch. 6). Political scientists should keep in mind that Brugger's interests are not "to describe economic behaviour . . . not to examine the economic determinants and implications of wage policy." Rather, he is posing political and sociological questions: "To what extent did the new system of economic administration reveal a contradiction between policy and resources," "the relationship between group and individual" and in chapter 6, "the effects of trying to implement a rigid staff-line system of command which derived from the Soviet Union." Chapter 7 then analyzes three institutions designed to serve as a check on management: the Factory Management Committee, the Enterprise Party Branch, and the Enterprise Union Branch. This process whereby checks were imposed upon management was part of a general process known as "democratization." The volume concludes with an effort to relate the processes analyzed in the volume to subsequent events.

The brief discussion above does not do justice to the intricacies of the work nor the thought and care reflected in Brugger's analysis. He is writing in an area where few have worked though he certainly takes account of research in related fields.

The volume is important for China scholarship as well as the problems of contemporary political-economic analysis. With respect to China scholarship, there is always some uneasiness about the quality of work. Rapidly changing events have occasionally impelled authors to write quickly only to find themselves outdistanced by events. Others have seen China as the special case without reference to the world in which the developing nation must live. Brugger has done a fine job of avoiding both of these problems. His scholarship, results, and interpretations are drawn with great care. Indeed, one wishes he had been willing to speculate more in his assessment of external influence, or likely consequences of specific policies. As noted above, the book would have profited from greater discussion of the Japanese occupation in the Northeast (Manchuria), particularly since this location was the testing ground for policies and programs gradually expanded to the country as a whole. The book is also valuable as graphic evidence of the careful scholarship and analysis that is possible utilizing case studies and economic handbooks published in the People's Republic of China.

Those whose research interests are outside the field of Chinese politics will find that Brugger's conclusions are of interest in the light of the historical experience with development of the Chinese and even more in terms of the apparent current priorities of the post-Mao leadership.

There are some annoying mistakes in the book and it could have profited from editorial work, both surprising in view of the generally high standards of Cambridge Press. Sentence structure is sometimes obscure, sentences often end with a preposition. Words are misspelled, "mixed" not "mized" (p. 257) or presented in the wrong form, "discussing" not "discussion" (p. 253). The first reference to the Movement to Create New Records refers to 1959–60 when it should have been 1949–50 (p. 7). The Lushun shipyard dates from 1892, not 1822 (p. 37).

Despite these errors, this is a fine piece of work.

JOYCE K. KALLGREN

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Strategy, Risk and Personality in Coalition Politics: The Case of India. By Bruce Bueno de Mesquita. (New York: Cambridge University Press, 1976. Pp. ix + 198. $17.50.)

"Without focusing on the unique character of Indian politics, the theory has succeeded in accounting for almost all of the variation in the change in [state] legislative strength of the All-India parties between 1967 and 1972" (p. 154). That is, this book organizes data from interviews with 88 non-Congress state party leaders and from state legislative elections between 1967 and 1971 so as to argue that party success in those elections depended significantly upon three factors: whether those party leaders had a high "need for achievement" (*n-ach*) relative to their opposite numbers in each state, whether those same leaders expressed a predisposition toward coalition-forming and coalition-maintaining strategies that combine a moderate degree of competition and cooperation with other parties (as opposed to highly competitive or highly cooperative strategies), and whether the party had done relatively well in the previous election and subsequent distribution of cabinet portfolios. The book organizes these generalizations in the intriguing argument that variations in electoral success in a multiparty context are largely determined by the parties' relative abilities to gain those key portfolios in the coalition government that are correlated with electoral performance, and that this ability is a function of *n-ach* and strategic preference (but not, interestingly, of size—though size does matter for a party's chances of entering a coalition at all). Generally, these relationships hypothesized by the theory did occur in the data, at statistically significant levels; in this sense the data provides empirical support for the theory.

This summary focuses on the book as a contribution to "positive political theory," which it claims to be. But from the perspective of specialists in Indian politics (to which status this reviewer certainly makes no claim), the book's interest may lie less in the general theory than in various digressions and case studies which illustrate the argument concretely. For example, some of the interview data is mined to yield the surprising suggestion that the opposition parties formed coalitions excluding the Congress, following the 1967 state elections, not on ideological but on merely power-seeking grounds; the meteoric rise of Charan Singh's party, the BKD, in Uttar Pradesh from 18 to 99 seats between 1967 and 1969 is argued to be due to the BKD's success in gaining drastic over-representation of key cabinet portfolios (seven times what their size would have entitled them); the different fortunes of the Jana Sangh in the elections following the dissolution of their coalitions in Bihar and Uttar Pradesh in the late 1960s is traced to the contrasting strategy predispositions of party leaders in each state; and the different electoral fortunes of the Socialists (SSP) and the Jana Sangh in Bihar in 1969 are traced to the great difference in the *n-ach* revealed by the two groups of party leaders in their interviews with the author. Moreover, it will doubtless matter to some specialists that the data of this study were derived not simply from one party or one state, but from six All-India opposition parties (two Communist, two Socialist, JS, and Swatanatra) in the seven states which had non-Congress coalition governments that included at least one All-India party; also, that the 88 cabinet ministers and top state party leaders constitute 74 percent of the total number of such leaders in all seven states for all six parties.

Specialists in "positive political theory" and in Indian politics, and those comparativists and methodologists who do not find the sentence quoted at the beginning of this review self-contradictory, will be able to decide from the above summary whether this book is likely to interest them. Other readers will find the book off-putting. The quality of writing and organization is often remarkably poor, especially just in those technical passages where clarity is most needed, such as the 45-page chapter (almost one-third of the book) reviewing the literature on coalitions and setting out the author's hypotheses. (Such a fault is in large part the responsibility of the editor of the press involved.)

Even the specialists, however, are likely to find the main argument of the book only suggestive at best, for two reasons. First, the book fails to connect its coalition theory to the major theoretical and empirical works in that field. Game theory is ignored entirely, and de Swaan's *Coalition Theories and Cabinet Formation* and Almond and his colleagues' *Crisis, Choice, and Change* (to mention the most obvious) receive no attention. Given these lacunae, the book is actually a contribution not to coalition theory, but rather to the study of the two social-psychological factors described above: *n-ach* and strategic predisposition. Second, there is often no explanation of obviously questionable features of the methods used, e.g., how important politicians could answer a 59-item questionnaire with sufficient care to reveal such "deep" personality traits as *n-ach*. Such questions may all be answerable, but it is a drawback that the book does not answer them.

MICHAEL LEISERSON

University of California, Santa Cruz

Total Revolution: A Comparative Study of Germany under Hitler, the Soviet Union under Stalin, and China under Mao. By C. W. Cassinelli. (Santa Barbara, Calif. and Oxford, England: ABC, Clio, 1976. Pp. 252. $19.75, cloth; $6.25, paper.)

The title of Cassinelli's book promises the reader a comparative study of revolution, as illustrated by the cases of Nazi Germany, Stalinist Russia, and Maoist China. But Cassinelli has not written a book that is rigorously comparative, nor one that focuses on the struggle for power in twentieth-century Germany, Russia, and China. His book does not generate new data or findings based on the cross-national testing of hypotheses. In short, it is not really a work of social science. Some will regard this observation as fatal; others will take it as an encouraging comment on the book's significance.

My own judgment is that social science, narrowly defined, was and is irrelevant to the task Cassinelli set for himself. He clearly wants to ask and answer questions that are not readily susceptible to hypothetical expression, operationalization, and cross-national testing with empirical data. He is never very far removed from "fact," in the sense of the ideologies and programs, the social and economic conditions, the power struggles and political structures appropriate to each of his three cases. But he is more interested in explanation than in description, and the questions in his mind more often begin with "why" than with "what."

Cassinelli has written a lucid and compelling essay in political history, an essay notable for its thematic unity and its consistently high level of interpretive insight. His important contributions to our understanding of Nazism, Stalinism, and Maoism is in terms of political ideas and their social consequences. And he is saying, in part, that the more radical the social change intended by the idea, the more grievous the consequences for humanity.

Cassinelli accepts the theoretical implications of this contemporary truism, and, in the context of his three cases, he gives it new and persuasive meaning. It is *idea* that proves most controlling in the unfolding of political history, at least when political history is written by leaders committed to "total revolution." And it is no coincidence that Cassinelli's three cases illustrate the consequences of concentrating power in the hands of leaders who define "truth" and "fact" in terms of the epistemology of idealism. Fundamental to the grandiose designs of Hitler, Stalin, and Mao was their rejection of the sensationalist epistemologies of modern science. Their revolt against liberalism, capitalism, and all the attributes of bourgeois society reflected their unshakable conviction that fact could be made a function of belief, that consciousness could transform existence, and that the will of the heroic leader could make objective reality conform to subjective intent.

Cassinelli does not use "total revolution" as a variation on the outworn and justly abused concept of "totalitarianism." "Total revolution" refers to the programmatic commitment of Hitler, Stalin, and Mao to reshaping the consciousness of their subjects by restructuring their environment. The ultimate objective (most closely approximated in Maoist China) was the union of leaders and the led through the unity of theory and practice.

At the root of this audacious enterprise was the leaders' assumption that their subjects longed for the security and satisfaction of community—whether defined in terms of race, nation, or economic class. It is not the specious individualism exalted by bourgeois society that responded to the fundamental compulsions of man's nature; instead it is the complete integration of the individual into his natural group that gives meaning to life and enables humanity to transcend egoism through a purified social consciousness.

Part of Cassinelli's achievement is in his demonstration of how the ideologies expounded and the revolutionary programs initiated by Hitler, Stalin, and Mao derived logically from their fundamental assumptions regarding epistemology and human nature. The book does not present a chronology of ideas and events or a catalogue of successes and failures together with their apparent causes. The book is a stunning example of reading meaning into history. The leaders' tactics may have changed from time to time, but their underlying strategies were a constant guide to their herculean efforts to remold mankind and society.

At the center of Hitler's, Stalin's, and Mao's political strategy was the principle of total power totally concentrated, but not for the sake of power itself. The *Führerprinzip* implemented by all three leaders reflected their conviction that they and they alone possessed the insight and vision that justified total power. The principle also explains their refusal to support the development of an efficient bureaucratic apparatus that was essential to military victory and economic modernization. This failure was a historical mistake of the first magnitude, but it was also an ideological and self-conscious choice: the bureaucratic efficiency that derives from hierarchy and specialization of task epitomized the bourgeois society that had been rejected in toto by the apostles of total revolution.

THOMAS H. GREENE

University of Southern California

The Dynamics of One Party Dominance: The PAP at the Grass Roots. By Chan Heng Chee. (Singapore: Singapore University Press, 1976. Pp. xiv + 272. Price not listed.)

Chan Heng Chee was a research fellow at the Institute of Southeast Asian Studies of the University of Singapore when she carried out the research for this book, which is based, we are told in a brief forward, upon her doctoral dissertation. The reader is thus fairly warned of the origins of the work, and should press on with the realization firmly in mind that dissertations seldom live up to their promise. This book, interesting and worthwhile though it is in some ways, is consistent with the general rule. It presents a detailed and competent monographic account of the People's Action Party of Singapore, as the subtitle indicates, at the grass roots. Taken as a whole, however, it is possessed of such serious logical problems that it cannot rise above the level of the monograph to make the broader contribution that the author suggested would be forthcoming, and that the reader hoped for.

The opening chapter sets out distinctions and definitions of one-party systems, and dominant party systems, and leads the reader to the purpose of this book, which is to describe and explain how the PAP has been successful in consolidating and maintaining its position as the dominant party in Singapore's political system. While this is to be a "micro-study" (p. 12), one is given to believe that it is to lead to the "clarification of some aspects of our theoretical knowledge of politics" (p. xiii). Three variables are to be deployed in this pursuit: "the organisation variable, the performance variable, and the opposition variable."

Following a brief history of the PAP's experience in power from 1959 to 1969, which serves as a backdrop to this study, one is introduced to five constituencies which are to be studied in detail. The subsequent chapters describe the party at the constituency level (the organizational variable), the party's management of local issues (the performance variable), and opposition movements in Singapore (the opposition variable). It is in these chapters that the best of the book is to be found. The sketches of the five constituencies are parsimonious and interesting to read, clearly the result of first-hand observation. The analytic chapters provide additional carefully organized detail about the politics of these constituencies, their party groups, organizations, concerns and desires. The chapter on opposition parties is especially good, providing considerable insight into the methods that the PAP has used to stalemate organized opposition.

In the last chapter, the whole argument disappears into a cocked hat as the reader is confronted by the rather surprising conclusion (no evidence having been presented on the point) that PAP domination is not so much a function of the organizational, performance and opposition variables at all, but the "political and cultural legacy of the island state" (p. 229), and especially of its Chinese community. "Paternalistic colonial rule by the British, with its centralized authoritarian decision-making and executive dominance" (p. 229), along with "Chinese political traditions, shaped predominantly by Confucian political philosophy" (p. 230), provides an explanation for PAP dominance which, while it may be plausible in itself, has no apparent connection to the rest of the book.

DONALD J. BAXTER

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Comparative Human Rights. Edited by Richard P. Claude. (Baltimore: The Johns Hopkins University Press, 1976. Pp. xvi + 410. $16.50.)

Research on human rights has traditionally been the preserve of legal scholars and philosophers. It is only in recent years that social scientists, especially political scientists, have become interested in the field. Most of their research has concentrated on public policy issues and on the efforts of international organizations to promote and protect human rights. This volume attempts to break new ground. It represents an effort to begin systematic, comparative research on human rights, admittedly among a relatively small group of countries, mainly western democracies.

The volume is an interdisciplinary effort, consisting of 15 previously unpublished essays written by political scientists and legal scholars, a psychologist and a sociologist. As might be expected in any enterprise of this sort, the essays are of uneven quality. The editor's introductory comments, which precede the essays grouped into the three main parts of the book, help in only a small measure to tie the essays together. The volume reads more like a collection of individual essays than a carefully integrated whole.

These are minor flaws, however. The book is very well organized; the essays are well placed within the three major parts. Moreover, the book succeeds in accomplishing its objective: it is explicitly exploratory in tone, and anyone interested in the field is bound to find in it a wealth of research ideas.

Part 1 (Political Legal Development) consists of two essays which complement each other nicely but which take fundamentally different approaches to the problem of the development of respect for human rights within states. In the first essay Claude provides a well-written, provocative "classical model" of human rights development. Drawing heavily on already existing historical, legal, and sociopolitical research on the major western democracies, Claude's model posits one "background condition," a "secure and procedurally regularized legal system" (p. 7), and identifies four problems which arise more or less serially on the road to respect for human rights: political freedom, legal guarantism, equality rights and participation, and positive rights policies. The problem here, of course, is that the model for the most part reflects human rights development in the major western democracies, and the question arises whether developments in nonwestern societies should be expected to conform to it. Claude demonstrates convincingly, however, how the model can be used to generate empirical as well as normative propositions regarding such matters as demands for citizenship equality, party elites, positive rights policies, etc. for comparative research.

The second essay, by James Strouse and Claude, takes a fundamentally different approach from that of the first essay. The emphasis is on cross-cultural analysis of human rights policies and social indicators worldwide. It is the only rigorously quantitative essay in the volume. Regrettably, though the authors clearly had no choice owing to the difficulty of obtaining reliable data, the essay is couched in highly tentative terms. The preliminary findings are interesting nevertheless—suggesting, for example, that rapid economic development tends to have an inhibiting effect on human rights development, and that several communications variables tend to have a positive impact upon rights relative to the electoral process. More research of this sort is needed.

Part 2 (Policy Problems in Comparative Perspective) constitutes the bulk of the volume. It consists of nine essays in which breadth of coverage was encouraged, not only as regards the countries selected (e.g., the United States, Mexico, Japan, India, Canada, several Western European countries, and several Scandinavian countries), but also as regards specific human rights policy issues (e.g., freedom of expression, the right to privacy, minority rights, racial discrimination, the status of women, and the rights of children). In each essay the author(s) attempt to place the human rights policies studied in their sociopolitical and economic setting. While each essay contributes in some way to the strength of the volume, readers might find most interesting and informative those by Manfred Wenner on the politics of equality among minority groups in several European countries; Lawrence Beer's study of freedom of expression in Japan with comparative reference to the United States; Anna Schreiber's study of the status of women in the United States and Scandinavian countries; and Linda Breeden's study of the rights of children, also in the U.S. and Scandinavia.

In Part 3 (New Directions in Behavioral Research), a marked shift in focus occurs. The emphasis is not on legal analysis of public policy as in Part 2, but on the behavioral dimension. Judith Gallatin's essay, for example, which is well worth reading, is concerned with exploring how adolescents in Great Britain, West Germany, and the United States evaluate and learn about human rights as opposed what their rights are in legal and technical terms. William Devall provides a very useful review of survey research findings on factors which contribute to support for human rights, and outlines the problems of theory building in the field.

The volume is a welcome addition to the literature of human rights. It would be a valuable addition to library and personal collections, and could be effectively used in specialized courses and seminars on the graduate level.

LAWRENCE J. LEBLANC

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Comparing Political Behavior. By Moshe M. Czudnowski. (Beverly Hills, Calif.: Sage, 1976. Pp. 224. $11.00, cloth; $6.00, paper.)

Accurate, general descriptions of this book are difficult to provide. There is a preface by Heinz Eulau and a foreword by Sidney Verba, both of whom attempt categorization. Verba states: "Rather, [Czudnowski] offers a sketch of a strategy comparative research" (p. 4). The variety of topics and arguments—objections to a science of politics, macro-quantitative studies of whole countries, voting behavior, public choice, and rationality—is so great that this book might best be considered essays on comparative politics.

Comparative research across levels, conceived as the analysis of socio-ecological space, is distinguished from that which stays at the level of country comparisons. The logic of cross-level analysis, which is outlined, is largely that of aggregates. The discussion of socio-ecological space is extended by the concepts of perceptual and behavioral space.

There is a chapter on voting and how voting studies can be informed by the perspectives of "system." Such an interpretation is one with which European social scientists have long sympathized. It is an example of linking micro-behavior to macro-consequences. Again this is a sketch of how this might be done and why, rather than a strong argument with a persuasive working-through of the example.

The last three chapters add up to Czudnowski's basic objective: a strategy for solving the problem of equivalence in comparative research. The underlying assumption is that a science of politics requires criteria for equivalence. Such criteria of equivalence will or ought to be derived from a general paradigm or a meta-theory of politics which, "ought to be sufficiently abstract to commulatively incorporate the findings of cross-systemic and cross-level comparisons, and yet sufficiently operational to allow for unambiguous rules for the interpretation of empirical observations" (p. 105).

The difficulty with a search for such a meta-theory is, of course, the trade-offs between an abstract paradigm or theory for incorporating variety, on the one hand, and precision of empirical interpretation on the other. The contradiction between the goals of generality and specificity is a major issue in comparative political research.

Exchange theory and the concept of public goods are suggested as foci for obtaining functional equivalence at the collective level. Czudnowski makes a theoretical distinction between those public goods that are exchangeable and those, such as legitimacy, that are not.

The major argument on equivalence comes in the last chapter on "Criteria of Substantive Equivalence": these criteria ought to be psychologically based. Czudnowski explores alternatives such as Lasswell's power motivation, then concludes: "but it is Maslow's parsimonious and hierarchical list of needs which lends itself best to a substantive interpretation of politics" (p. 145). One may of course question how firm an anchor such a specific psychological view of the nature of the individual is.

The logic of Czudnowski's general strategy is familiar: in order to cope with the diversity confronted in comparative political research, we must identify some constants. These constants, a meta-theory, can provide a base line for understanding complexity.

What has been learned from empirical research in comparative politics during the past three decades can perhaps be summarized by two very general statements. First, micro-level findings, particularly at the individual level dealing with relationships among such variables as education and urbanization, tend to be relatively similar cross-nationally. Second, the ways in which these configurations of micro-level phenomena aggregate or compose into macro-level phenomena are considerably disparate. India, the United States, and Germany, as political units, are very different from one another, whereas their modes and styles of individual political participation—elections, parties, and voting—may be relatively similar.

An earlier paradigm or language for establishing substantive criteria of equivalence, in order to organize the observed variety at the political system level into comparable dimensions, were concepts inspired by functionalism and cast into the traditional categories of political science: rule making, rule adjudication, etc. Such concepts did not solve the problems of complexity by providing for both generality for comparisons and accuracy for explanation.

To shift from the constants of functions at the system level to constants of psychology at the individual level is suggested as a way out of the mess of the observed increasing variety at the macro-level. To achieve consensus on a psychological paradigm is a weak hope; consensus on a particular psychological model is hopeless.

The alternative to finding or establishing a paradigm for anchoring the complexity in comparative political research is to deal with complexity by complex modes of analysis. Social scientists are perhaps not yet prepared to do this. However, complex models to reflect complex patterns may be in the long run a more viable alternative than searching for substantive criteria of equivalence to deal with reality by simplication through constants that may be simple only in the sense that their number is small.

HENRY TEUNE

University of Pennsylvania

Electoral Participation in a South Indian Context. By David J. Elkins. (Durham: Carolina Academic Press, 1975. Pp. 251. $10.00.)

In the literature on political participation, most studies have focused on individual-level variables (education, age, sex, sense of efficacy, etc.). Investigation of ecological or community-level variables (election rules, social structure, community development level, etc.) has been less common. Of course, both the individual and ecological variables as determinants of political behavior must be systematically studied before we can gain an adequate understanding of political participation. All behavior cannot be explained merely in terms of characteristics "internal" to the individual, for individuals with virtually identical personal characteristics living in different regions may participate at different levels.

David Elkins' study is a useful departure from the dominant individualistic approach to political behavior. His focus is on aggregate community-level variables like the degree of linguistic and social diversity, the pattern of status hierarchy, and the degree of party competition in the South Indian context. Much of the information on these variables is drawn from the 1961 Indian census, and some from historical, ethnographic and linguistic data. The data on party competition, voting turnout, and on the average number of candidates per constituency are taken from the *Reports* of Indian Election Commission in four South Indian states for the period 1952–1967. The unit of analysis is the district (much like the county in the United States). This procedure is necessary in the Indian subcontinent because census data are available only at the level of the district, and not for electoral constituencies.

What are some of the strengths of the book? First, Elkins takes an innovative approach in using census, historical, and anthropological materials in understanding electoral participation in India. Using these materials he is able to study the impact on participation of such novel variables as caste fragmentation, caste dominance, social diversity regional communications, regional literacy, regional development, and rural vs. urban character of the district. His major findings can be summarized thus: regions showing higher voting turnout are characterized by lower social diversity, lower caste fragmentation, heightened party competition, more widespread regional communications (number of newspapers, extent of surfaced roads, etc.), higher literacy, and higher agricultural development.

Elkins' focus on ecological variables helps us interpret a very high voting rate for an underdeveloped country. There are low levels in India of those personal factors which have been found in the West to correlate significantly with voter participation–factors such as education, knowledge, information, sense of efficacy (cf., L. W. Milbrath and M. L. Goel, *Political Participation*, Rand McNally, 1977). Yet voting turnout is impressively high. On the average, 60 percent of the nationwide electorate participates in state elections (Kerala: 85 percent, in 1960; Tamil Nadu: 73 percent in 1967). What explains this activism? Part of the answer to this question lies in the liberality of legal requirements for voting in India. Few people are barred from exercising their franchise because of their sex, race, religion, caste or language, or residence. A major factor in the equation is also the ease of registration. Unlike voluntary registration in the U.S., which often requires a trip to the county courthouse, the Election Commission in India conducts a door-to-door compilation of eligible voters. Elkins' contribution lies in highlighting the role that social organizations and group leadership play in increasing turnouts. Illiterate and ill-informed citizens become mobilized as a result of pressures put on them by work team leaders, dominant caste groups, and village elites. Thus, election-day turnout is less a result of internal motivation and more a result of external mobilization.

The book is methodologically self-conscious. Research based on ecological factors can often be open to *the criticism* of "ecological fallacy," i.e., of making inferences at the individual level from data measured at the aggregate level. The author has avoided this pitfall. The future researcher interested in problems of aggregate data analysis in the subcontinent and in matching census data with district data may find the book instructive.

The book has one major limitation because of the data used. It is limited to the discussion of a single type of participation–electoral participation. Recent research demonstrates that there are several different "modes" or "styles" of political participation (voting, campaigning, contacting officials, protesting, communicating, community work). These modes are alternative participatory strategies that citizens pursue in their relations with the government. All these modes are related to one another and to the general dimension of political involvement. *Voting, however, is least closely related with the general dimension of political involvement* (see Milbrath & Goel). Therefore, one should be cautions about inferences about political participation based on voting data alone. While it may be true that rural dwellers turn out in larger numbers than urban dwellers, it may still be true that urban dwellers exceed rural dwellers on more demanding forms of participation (discussing politics, party work, community involvement, attending rallies, participating in protests).

Unhappily also, the book is so formally organized and written that it makes reading tough and laborious. Beginning students, without methodological training or familiarity with the new jargon will find it slow reading. It seems that in political science only a few can execute a methodologically sound and yet a readable text.

M. LAL GOEL

University of West Florida

L'Algérie, cultures et révolution. By Bruno Etienne. (Paris: Editions du Seuil, 1977. Pp. 333. Price not listed.)

This extremely well-informed analysis of Algerian strategies of national integration raises more questions than it can answer about the ruling elite and its relationships with various social sectors. While equally critical of Marxist analyses of this elite and of official Algerian denials of class struggle, the author insists on labeling the elite a "ruling class" but then adds that it is a "class" only by virtue of occupying key decision-making positions in the state apparatus (p. 61) and that the "middle classes" from it was largely recruited "did not in the least constitute a homogeneous class" before independence (p. 50). Yet the "ruling class" has already become (p. 299) or is in the process of becoming (p. 225) a "dominant class" by virtue of its ability to impose its own system of values upon the society. Just as it acquired control over most means of industrial production, it has virtually monopolized the means of cultural expression. It exercises "symbolic violence," appealing to science but also affirming "ethnocentric" values for the masses whom capital-intensive industrial investments cannot benefit in the immediate future. Agrarian "revolution" by the same token is interpreted as a device to keep rural masses from infesting the cities. But do regime policies reflect the coherent strategy of a dominant class?

No convincing evidence for the existence of such a class is presented. To be sure, economic inequality exists in "socialist" Algeria. But however much conspicuous consumption in Algiers shocks the author (p. 83), it reveals, as he finally concludes, only that some people, not necessarily decision makers, are making money (p. 313). It is also true that the state is the principal employer and accumulator of capital, but Etienne does not infer that those who control the state constitute a bourgeoisie. By virtue of cooperating, necessarily, with some multinational companies, they might be deemed the "accomplices" of international capitalism (p. 244), but the author rightly objects that Algerian industrialization based on the rational exploitation of its petroleum and gas resources is intended to break the chains of dependence.

Etienne still speculates that a bourgeoisie based on a growing private sector might gradually translate its economic position into political power, leading as in Egypt and Tunisia to a "denationalization" of some of the more lucrative parts of the public sector (p. 301). But such a class could not be the ruling one he describes as bent on "preventing any other stratum from becoming a new dominant class" (p. 304). He also suggests that new forms of "clientelism" may indicate the formation of a class within the bureaucracy (p. 91), but he does not develop this potentially interesting line of argument. He postulates "a relationship between the social stratification developing in Algiers, the practices and ideology of the consumer strata and the internal and external option of the regime" (p. 117), but the evidence seems to point instead to the regime's relative autonomy.

Despite problems of theoretical coherence and balance, this book is an exceptionally open and honest account of Algerian political strategies and deserves the attention of students of comparative politics and revolution in the third world. Whether in the name of science or "ethnocentric" values, the book has apparently been banned in Algeria.

Clement Henry Moore

University of Michigan

Rogues, Rebels, and Reformers: A Political History of Urban Crime and Conflict. By Ted Robert Gurr. (Beverly Hills, Calif.: Sage, 1976. Pp. xii + 192. $10.00, cloth; $5.95, paper.)

This short volume develops the conceptual foundations and summarizes the principal conclusions of a large research project headed by Ted Gurr on the history since 1800 of crime and civil conflict in four cities—London, Stockholm, Sydney and Calcutta. The overview volume, a more extensive, multi-authored work which includes detailed case studies of the four urban centers appeared in early 1977, apparently was prepared for a general audience of nonspecialists. (Ed. note: This volume, *The Politics of Crime and Conflict,* is reviewed below.)

The study had two broad objectives. Descriptively and empirically, Gurr and his colleagues hoped to learn whether the incidence of individual crime (murder, assault, theft, etc.) and collective strife (rebellions, riots, strikes, etc.) are systematically connected, and, whether as is commonly believed, crime and civil strife declined in magnitude throughout the nineteenth and first part of the twentieth centuries, then increased appreciably from the mid-twentieth century. At the explanatory and theoretical level, the study attempts to identify and assess roughly the relative importance of socioeconomic, political and institutional factors in the creation and maintenance of public order.

The empirical data assembled during the course of the project include official statistics on the incidence of individual crime, more limited data on riotous behavior and assaults on police, and narrative information on major episodes of civil strife. (Statistics on strike activity were also collected, but these data are not exploited in this summary volume.) The quanitative analyses of public disorder are therefore confined largely to trends in the incidence of common crime. Even here the results may be viewed with skepticism, since crime statistics vary considerably in scope and quality. I am inclined, however, to accept Gurr's judgment that the limitations of the quantitative data are not serious enough to impair the major conclusions of the study, especially since no attempt is made to compare crime rate levels across the four cities.

The results of the trend analyses confirm the common belief: crimes against persons and property fell sharply during most of the nineteenth and first half of the twentieth centuries, but then increased significantly after the 1940s or 1950s. In this respect, as in others, Calcutta is an exception to the pattern observed in the three "western" urban centers—the crime rate in that city exhibits no discernible long-run trends. Gurr argues, persuasively in my view, that the dramatic reversal of the 150-year trend in the crime rates of London, Stockholm and Sydney is not a statistical artifact, but a reflection of real changes in the incidence of criminal behavior. Unfortunately, Gurr is not able to supply us with an explanation; the secular increase in urban crime since the second World War remains a puzzle.

Gurr's discussion of the historical record of collective strife in the urban centers is necessarily more impressionistic than the analyses of crime rate trends. He finds little evidence in favor of the model proposed by Charles Tilly in which civil conflict evolves sequentially through "stages" characterized by different motives and modes of social organization. By Gurr's account, most strife, in the western cities at least, has been what Tilly would call "modern" for more than 150 years, i.e., has been initiated by associational, class-based groups seeking greater political rights and economic benefits. Although in the short run such collective actions generally encountered stubborn and sometimes violent resistance from elites, in the long run accommodation and reform prevailed as "the locus of political power has moved down the class ladder..." (p. 79). However, Gurr is cautious about projecting a future of civil peace for urban, western societies—new sources of conflict inevitably arise and there is little guarantee that established systems of conflict management will peacefully accommodate them.

Does it make sense to treat forms of common crime and events of collective conflict as relatively homogeneous phenomena that can be usefully classified and analyzed under the single concept "public disorder"? In other words, do, say, homicides and riots have enough in common to merit a unified investigation? In my view, the answer is no, but Gurr takes pains to establish a reasonably close correspondence between the "two faces of disorder" and no doubt many readers will find the argument convincing. Gurr argues that public officials often perceive and react to both types of disorder in similar ways, and he appeals to a number of theoretical traditions suggesting that individual crime and civil conflict have common root causes. Empirically, his case rests primarily on quantitative analyses of the short-run association between "crime waves" and major episodes of civil strife. He concludes that "sharp increases in indicators of crimes of violence and theft usually coincide with episodes of strife," which is seen as a "pervasive and important social phenomenon" (p. 90). Space does not permit me to review these analyses in any detail, but I think Gurr's conclusions are much too strong in light of the available evidence. Indeed if ordinary labor strikes and the anti-U.S./Vietnam demonstrations in Stockholm (which were viewed sympathetically by the Swedish Social Democratic Government and on at least one occasion included the participation of Olof Palme, then Minister of Education, subsequently Prime Minister, who marched along with representatives of North Vietnam) are excluded from the civil strife tally, I do not see any sign at all in Gurr's data tables of a linkage between short-term changes in the incidence of common crime and collective conflict.

Notwithstanding my reservations about some of the conceptual and quantitative material in this book, the core chapters treating "elite interests and public order," "police and policing" and "trials, punishment and alternatives" are masterful illustrations of comparative, historical analysis. Let me give just a few examples from the chapter on police. Gurr makes insightful criticisms of modernization theory—the modernization of police organizations has been more a matter of coordination, integration, and professionalization than a shift from functionally diffuse to functionally specific organizations, as academic theories of institutional modernization or development would have us believe. Gurr uses class analysis to advantage—the nineteenth-century expansion of police forces reflected the convergence of the middle-class interest in protection of person and property and the elite interest in security from collective challenges from the lower classes, which was sufficient to overcome the fear of the middle class that police might be used against them and the aversion of both classes to the growth of public expenditure. And he draws some rather striking policy implications from his empirical results—increased police manpower did not appreciably retard the postwar secular rise in crime.

In the final chapter of the volume Gurr outlines a theoretical model of public disorder by sketching the patterns of association among various behavioral, environmental and institutional factors. This section reflects the ambiguous picture emerging from the comparative, historical parts of the study and as a result is perhaps the weakest chapter of the book. Gurr is, of course, aware of the limitations; he offers the crude, theoretical model only as a rough guide to future work. There is no doubt that subsequent investigations will profit substantially from this volume. I learned much from reading *Rogues, Rebels, and Reformers* and I encourage others to share my experience.

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The Politics of Crime and Conflict: A Comparative History of Four Cities. By Ted Robert Gurr, Peter N. Grabosky, and Richard C. Hula. (Beverly Hills, Calif.: Sage, 1977. Pp. xii + 793. $35.00.)

In 1800, London police reformer Patrick Colquhoun wrote of his hope "that his humble endeavours, for the good of his Country, will ultimately produce arrangements *in the new Science of Police,* calculated to secure and protect the peaceful subject against injury, and to ameliorate the state and condition of Civil Society, particularly in this great Metropolis. . . ." Like his Parisian counterpart Nicolas de la Mare a century earlier, Colquhoun advocated the strengthening, extension and centralization of police jurisdiction over a wide range of municipal activities. Unlike de la Mare, however, Colquhoun was an innovative general theorist who built up an important set of arguments concerning the ways in which the social structure of the metropolis encouraged crime. In those arguments, Colquhoun anticipated three of the four standard brands of criminological theory still in use today; social-control theories, moral-decay theories, and opportunity theories. The only variety the self-confident Scot missed was labeling theory, which proposes that "crime" and "criminal" are labels which powerful people attach to deeds and persons they dislike.

Ted Gurr and his collaborators are successors of Colquhoun the theorist, if not of Colquhoun the police reformer. Their scale is larger. They deal with four cities: not just London, but also Stockholm, Sydney and Calcutta. Concerning themselves with collective protest and conflict as well as individual crime, they want to draw their conclusions concerning the general determinants of public order and disorder from close analysis of nearly two centuries' experience in four great cities. Their explanations incorporate not only the contemporary versions of social-control, moral-decay and opportunity theories, but also some labeling ideas. For Gurr, Grabosky and Hula, the changing structures of cities generate crime and conflict, all right; but "elite goals and interests" significantly affect the legal definitions of crime and unacceptable conflict. Elite goals and interests thereby shape the prevention, surveillance and retribution directed at public disorder.

The great bulk of this 800-page book, however, consists of straight forward description. Gurr and his many collaborators have prepared lengthy summaries of the published literature concerning policing, reported crimes, and civil strife in London, Stockholm, Sydney and Calcutta since about 1800. The information on civil strife which they survey is much more fragmentary than the information on policing and crime. Nevertheless, the various research squads which worked on each of the cities were generally able to assemble enough information for the statement of tentative answers to the following questions: Is it true, as has often been alleged, that western cities became more orderly in the nineteenth century, and that the trend has reversed during the last few decades? If so, why? How important a part did police forces and other institutions of social control play in whatever changes occurred? What about the role of elites in defining "disorder" and producing new efforts to control it? What was the place of civil strife, as distinguished from individual criminality, in all these changes?

These are important questions. The answers offered are mixed and provisional. On the whole, reported criminality—especially property crime—did tend to decline in the western cities during a substantial part of the nineteenth century, and has moved up again in recent years. (At several points, Gurr and collaborators begin to develop an argument which stresses the role of material necessity in the nineteenth century, as opposed to the increasing importance of opportunity to steal in the twentieth century; since the two observations are not really contradictory, it is a pity they did not try to bring them together into a unified model.) The evidence is more mixed for crimes against persons. As for collective violence, Gurr et al. find fluctuations, but no long-term trends in individual cities, and no common patterns among all the cities. Policing and punishment affected the reporting of different sorts of crime, but how much they affected the actual frequency of crime remains quite unclear. Elites did have considerable influence over the prevailing definitions of "disorder" and over the use of force to check crime and protest; the creation of centralized, professional police forces, for example, depended to an important degree on the local elite's perception of a threat of protest or rebellion.

The generalizations about trends do not apply to Calcutta; Richard Hula is so skeptical about the relationship between the crime rates and "true" criminality in Calcutta that he uses the rates as an index of elite attitudes toward disorder. Hula provides an interpretation of the fluctuations in reported crime as a function of the elite's concern about one form of threat or another to its position. When police moved into street-clearing operations against popular protests, for example, arrests for public order offenses shot up but reported rates of individual assault went down. The concerns of the elite and police, in turn, grew from the city's political history—most importantly, from the growing resistance to the British colonial government.

The joining of political history to the analysis of crime and civil strife is the most attractive feature of this book. The book also has some unattractive features: the vagueness of its theoretical formulations, the dullness of its prose, the frequent burying of the argument in the sands of detail, the general lack of discipline in the inquiry. The final model which emerges from all this observation and reflection is embarrassing in its poverty. It consists of a conventional list of factors directly or indirectly affecting the form and frequency of public disorder. Direct influences: police and judicial systems and policies, extent of social disorder, legal definitions. Indirect influences: elite goals and interests, imperatives of the economic system, social scale and cultural heterogeneity. The list appears in the form (but not the substance) of a path diagram stating the direction and immediacy of the relationship between each pair of factors. A flabby, uninteresting outcome—the more so because it does not lead to any very promising agenda for theory or research.

Such an outcome is embarrassing because it tells us how little we have improved on Patrick Colquhoun. The energetic Scot did not have the benefit of extensive crime reporting, a massive comparative analysis or multivariate statistical analysis. But he did have a relatively coherent and verifiable theory of public order which was rooted in the everyday practice of policing. At the end of all this compiling and reflecting, Gurr and his collaborators do not.

CHARLES TILLY

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The Political Economy of Contemporary Africa. Edited by Peter C. W. Gutkind and Immanuel Wallerstein. (Beverly Hills and London: Sage, 1976. Pp. 318. $17.50, cloth; $7.50, paper.)

Some time ago A. G. Frank and other Latin American economists articulated a view of underdevelopment which has come to be known as the "dependency" thesis. According to this perspective there is a reciprocal, dialectical, relationship between the First and Third Worlds: the relative development of the First has produced the underdevelopment of the Third, and the underdevelopment of the Third was a condition for the development of the First. Thus it is a mistake, the "dependentistas" would argue, to search in the "isolation" or "tradition" of the Third World for impediments to "growth." The Third World has long ago become "integrated" into the world economy and has become "modernized" in the sense that it is the poor, unstable, and exploited periphery of capitalism. All 12 essays in this volume, including the introduction by the editors, assume the dependency approach or as Peter Gutkind states: "Why is Africa so poor? . . . the answer is very brief: we have made it poor" (p. 27).

The great strength of neo-Marxist analysis as it applies to Africa is ably represented by the international panel of scholars, four of whom are Africans, who make up the contributors to this volume. They all share a perspective which resists the differentiation of political reality into "politics" and "economics" or into "comparative politics" and "international relations." They understand that such divisions may be analytically useful in the short run, but that ultimately, the social and political world should be understood as a whole. Some of the weaknesses of this perspective are also evidenced in this volume. Briefly, the two main weaknesses consist of an inability convincingly to produce the linkages or rules of transformation between international factors and domestic events, on the one hand, and to fit a sometimes recalcitrant reality into a preconceived class analysis, on the other hand.

Turning to the problem of linkages between "the world system" and Africa, Wallerstein, who is a distinguished Professor of Sociology at the State University of New York, Binghampton, sees the slave trade as the "cutting edge of the peripheralization of Africa" (p. 34), and sees colonialism as the European response not only to the needs of capitalism for markets and commodities, but also to the fear that an African bourgeoisie might arise and come "to utilize the local state structures to erect mercantilist barriers" (p. 37). The late flowering of nationalism he relates to the slow evolution of a middle class whose rate of development was retarded by the lag of world industrial production behind African raw material production. The strength of his analysis is that it links the slave trade to colonialism, colonialism to nationalism and all three phases to the world economy.

Though Wallerstein's landscape is painted in bold, broad strokes and it may be to cavil, but what are we to make of it when he argues that by the mid-60s the fact that world raw material production lagged behind industrial production accounted, "in large part," for the decolonization of Portuguese Africa (p. 48)? Had the MPLA nothing to do with it? How is one to make the connection between "production in the world system" on the one hand and political events, such as "decolonization" in Africa on the other hand? Perhaps Wallerstein has rules of transformation elsewhere, but he does not demonstrate them in this essay.

Similar problems can be found in some of the other chapters, especially that of T. Szantes, who has written a broad-gauged paper on socioeconomic effects of foreign capital investments.

If the writers who try to account for endogenous African developments by way of exogenous factors need to create convincing linkages between the two, the writers who deal with internal African developments by means of class analysis risk forcing a recalcitrant reality into the procrustean bed of Marxist historicism. In particular, Robin Cohen who is a Senior Lecturer at Birmingham University, works mightily to stuff the African worker into the uniform of class-conscious proletarian. Ethnicity and patron client relations may indeed be obstacles to class formation and class consciousness, but without understanding their existential reality for the African worker, one fails to understand him. It is quite wrong too, I think, to suggest that increasing industrialization and urbanization in Africa is going to lead to "peasantization" and "proletarianization," in the sense that ethnic and other communal ties will become less important. Indeed, as the insecurity of capitalist or socialist or state-capitalist development increases one would expect that such ties would multiply and become stronger. In brief, the concepts of "class" and "class consciousness" applied to the African setting can be useful so long as they are not reified; when they are, they become mystifications like any other.

Two of the more provocative essays are by Gavin Williams and Claude Ake. Williams asks us not to treat the African peasantry as a doomed class. When he has not been coerced by absentee landlords and incompetent bureaucrats, the African peasant has admirably produced the surplus so necessary to the industrializing sector. Williams' thesis, of course, contradicts the observations of Marx, Lenin, and most recently Barrington Moore, Jr. As for Claude Ake, he has written a brilliant paper, reminiscent of Fanon, linking shifts in African ideology to phases of African economic and political development. These two essays alone are worth the price of admission. The volume ends with a bibliographic essay by Chris Allen. Except for leaving out the work of Elliot J. Berg, one of the most talented and trenchant critics of the socialist position on Africa, the essay brings to light new work in political economy and is well done.

ROBERT MELSON

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Soziale Bedingungen für Rechtsextremismus in der Bundesrepublik Deutschland und in den Vereiningten Staaten. By Thomas A. Herz. (Meisenheim am Glan, W. Germany: Verlag Anton Hain, 1975. Pp. iv + 283. Price not listed.)

Modern industrial society equals "rapid social change": incantation of conventional wisdom or explanatory paradigm for the roots of right-wing extremism in modern industrial societies? Is the traditional middle class in contemporary societies psychologically disoriented, socially uprooted, politically alienated, ideologically paranoid, etc., etc., and ready to follow pied pipers who promise to make things right again (George Wallace, Enoch Powell, Pierre Poujade, the Soku Gakkai, the NPD—the specter of Nazism looming the background)? Conventional wisdom does have its pitfalls, however, and comparative analysis brings them into sharper focus. At the very least, the alleged intervening mechanisms between "rapid social change" and support for right-wing extremism must be made operationally explicit if conventional wisdom is to be transformed into social research or, for that matter, theory.

Herz attempts to do just that. He presents a verbal model of right-wing radicalism proposed by Scheuch and Klingemann, in the spirit of earlier writings (Parsons, Lipset, Geiger) and he develops his analysis with data from the 1968 Survey Research Center election study in the U.S. and a specially commissioned 1969 DIVO survey in Germany. His linkages between theory and data vary in conceptual sophistication, and the data, appropriately, reward some of his assumptions and punish others. The more serious problems arise in merging individual and social concepts and in linking latent and manifest conditions. Herz does recognize them; but he fails to resolve them. His "social psychology" and "political sociology" models remain unconnected, and only in the former does the shadow of "rapid social change" remain discernible in the background. But then, it also does a lot worse in accounting for right-wing support than his "political" model, which is both more interesting and more problematic theoretically. It suggests, in effect, that support for right-wing issues, candidates, or parties is blocked as long as individuals or groups believe that their social and economic interests (and the norms surrounding them) are being protected by established political parties and institutions. Failing that, right-wing extremism becomes a rational pursuit of material interests, an instrumental response to current issues (an interesting analysis of the phenomenon, unencumbered by the perspective of the surgeon about to operate on a carcinoma).

Herz's specification unfortunately blurs the distinction between (1) latent affinity for right-wing issues or ideology, and (2) actual support for right-wing candidates or parties. While he set out to analyze the sources of (1) and the conditions of its conversion into (2), he stopped short of stating the problem in these terms. He introduces measures of right-wing ideology, social integration, and crisis perceptions as interaction variables. Yet his data show that ideological affinity and political support follow quite different imperatives. In Germany, for example, right-wing potential is considerably higher among the *less* educated segments of the public, while its rate of conversion into support for the NPD is much higher among the *more* educated; in the U.S., it is exactly the other way around. Surely it is difficult to resist the temptation of seeking to uncover facilitating or inhibiting mechanisms beneath these highly suggestive patterns. Such a model might even have offered a unified framework for the analysis of similarities and differences in the mobilization of right-wing support during the 1960s.

Nevertheless, as a comparative empirical study, the monograph offers a series of thoughtful and stimulating insights into popular support for Wallace and the NPD, in addition to the path analysis of alternative structural models and the inevitable factor analysis of everything (no egregious statistical errors are in evidence, though a number of minor flaws might irritate the technical reader). In sum, Herz's thesis does not provide a model of support for right-wing extremism in contemporary western societies. But it does manage to clear away some of the conventional underbrush.

LUTZ ERBRING

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The Model Ombudsman: Institutionalizing New Zealand's Democratic Experiment. By Larry B. Hill. (Princeton, N.J.: Princeton University Press, 1977. Pp. xviii + 411. $18.50.)

This Scandinavian invention, the citizens' grievance man, offers the average person a link to the bureaucracy to insure that fair treatment is received from administrative authorities. This book is a study of the transfer and institutionalization of the ombudsman into the New Zealand political system. In retrospect, politically stable New Zealand, with its tradition of social innovation, provided a fertile field for such transfer. The ombudsman was congruent with existing political values, which included responsiveness to citizen concerns—handled by members of Parliament who were in close touch with constituents, ministers who were accessible, government departments, parliamentary petitions and administrative tribunals.

The ombudsman concept, incorporated in the National Party election platform in 1960, was transferred to New Zealand two years later. Opposition at the time was expressed by the Public Service Association, and the opposition Labor Party sniped at the proposal, but did not actively oppose its adoption.

The author has used what he characterizes as a political-anthropological method by immersing himself in the culture in order to record observations. The researcher is neither the outsider nor the insider—he retains his objectivity but brings an understanding of the environment to his research. His 19-month field-based study, in which he wrestled with the twin problems of ombudsman performance and institutionalization, began in January 1967 and incorporated interviews, statistical analysis and case studies—including an intensive analysis of 450 sample cases from the period of 1962–1969.

A great deal of good research, much of it normative, had already been undertaken—major studies were published by Walter Gellhorn and Stanley Anderson. Hill's contribution is his empirically based research and his focus upon the process of institutionalization, which he defines as "a process that occurs over time in which the organization creates authority relationships vis-à-vis the environmental actors" (p. 24).

This book includes chapters dealing with institutional relationships that have developed between the ombudsman and various other actors in the political environment—bureaucrats, members of Parliament, ministers, and citizens. He uses Samuel P. Huntington's conception of institutionalization as measurable by two internal, structural, criteria—complexity and coherence—and two environmental criteria—autonomy and adaptability. Measured against these criteria, Hill argues that the office has been successfully institutionalized. Although it was a small office for the first decade of its existence and therefore not complex, modifications triggered by 1975 legislation will transform it into a more complex organization. The Ombudsman Office has been highly coherent, has been autonomous from the start—due to commitments from both political parties not to intervene, and is showing itself to be adaptable, having served under two different prime ministers from each party. A further test will be how the Ombudsman adapts to its increased workload having assumed jurisdiction over local government administrative decisions in 1975, and to a more complex organizational structure, including a troika of ombudsmen.

There are other interesting findings—some of which are corroborated by other researchers. The functioning of the ombudsman does not disrupt administrative agencies, nor are complaints seen as threatening to departments since they raise individual grievances and are not focused on personnel. Members of Parliament do not perceive the ombudsman as a rival in handling constituent complaints, nor do ministers believe that their responsibilities for the operation of government departments have been eroded or threatened. By far the largest number of complaints are directed at what Hill terms client-serving agencies—social welfare, education, and health—the products of New Zealand's well-developed social welfare system. Complaints against the client-attending organizations—justice, labor and police—are fewer, with very few complaints against non-client-oriented departments such as the Ministry of Works.

Contrary to what some of the ardent ombudsman advocates suggest, most complaints are not directed against bureaucratic harrassment or unwarranted intrusion into private lives. Rather, "the great bulk of the clients are identified as competent, responsible, ordinary New Zealanders with very real problems" (p. 129). Complainants want help in obtaining something, frequently financial, that they believe to be rightfully theirs and do not seek protective action from department decisions.

Procedural and substantive policy changes occur beyond the investigations of an individual complaint. Based upon the ombudsman's own 863 reported case notes during the period 1962–1975, the author concludes that there were "at least 177 separate reforms of the administrative process" (p. 210). Over half of these impacts were substantive. The author does not delve deeply, however, into the significance of these incremental and salutary changes.

Although much of the contents of this book has been published previously in separate articles, this composite work is valuable for ombudsman students. Hill perhaps overstates his claim to having been a pioneer in his research techniques. Empirical research was being undertaken concurrently with his own in 1967 regarding organizational relationships and the ombudsman's interaction with other actors—public servants, members of Parliament and ministers. His subsequent compilation several years later would have afforded a good opportunity for comparison of results. If there is a fault in this study, it is that of missed opportunities. The book contains several appendices including a statement from the official reports of the New Zealand Ombudsman, Sir Guy Powles, who has served for many as the model ombudsman. Hill also provides a selected bibliography. This is a well-thought-out and analytical study based on field-based research that provides the data and the conceptual framework that could be applied to other comparative ombudsman studies.

KENT M. WEEKS

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Social Limits to Growth. By Fred Hirsch. (Cambridge, Mass.: Harvard University Press, 1976. Pp. x + 208. $10.00.)

In both his title and his introductory chapter, Fred Hirsch consciously seeks to juxtapose his arguments on limits to economic growth with those of the Club of Rome. When he says that the Club of Rome "focuses on distant and uncertain physical limits," he does them an injustice, and ignores the communality of his and their arguments. The club has in the second report (*Mankind at the Turning Point*) and in its meetings and statements emphasized the limits to consumption as well as the limits to production. The emphasis on the consumption side of the issue has surfaced not only in the club's concern with the environmental impact (or externalities) of consumption, but also in its questioning of economic models of growth which lead to such absurdities as two or three automobiles per capita in developed countries predicted by the turn of the century.

More interesting and important, however, is Hirsch's apparent belief that his focus on consumption distribution is somehow foreign to the international organization. The distribution of income and consumption has been, in fact, the major focus and concern of the second and third (*Reshaping the International Order*) reports. Where Hirsch does go beyond the Club of Rome, and breaks new ground much more generally, is in his emphasis on the issue of inequality within the developed world and in his original arguments showing the importance of inequality as a growth-limiting factor.

The argument of the book is economic and centers on the distinction between public and private good. He goes considerably beyond Galbraith's recognition of an imbalance and also beyond Olson's explanation for imbalance in the *Logic of Collective Action*. Hirsch argues that the pursuit of private goods by some individuals increasingly affects the ability of others in society to obtain what they want, thus blurring the public/private distinction. The key to the argument lies in the distinction between material and positional goods. Until recently, private goods (such as food, cars, televisions, houses) were sought by individuals in developed countries, and the market mechanism very successfully transformed individual behavior into bountiful supplies for all. But as we have thus satisfied the lower, survival needs in Maslow's hierarchy, we have turned to the higher, including self-esteem. Thus the goods sought have become art masterpieces, waterfront property, and satisfying employment—positional goods, very often, by definition, in limited supply. The zero-sum game pursuit of such goods often proves both wasteful and inflationary. The way out would seem to be in a new social morality, in which positional goods become both less intensely sought and better distributed. Hirsch, however, builds his case for social limits further by arguing that the individualistic basis of capitalism has greatly weakened religion-based social morality so that even collective behavior (e.g., unions) has an individualistic basis rather than a basis in social value.

In spite of the fundamental indictment of capitalism, Hirsch offers no revolutionary prescription; in fact his work is primarily an academician's hope that the problem will be more widely recognized and further defined. Nonetheless, the book is original, penetrating, and challenging. It offers a set of concepts with implications the reader automatically begins to extend beyond Hirsch's illustrations. For instance, could the increase of nationalist and separatist sentiment in many developed countries be partly explained as pressure to expand the supply of positional goods by creating more and smaller ponds to elevate additional individuals to big-fish status?

The reader should approach this work as a serious and scholarly treatise, and not as either a text or a popular exposition. It is well worth reading.

BARRY B. HUGHES

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Revolutionary Law and Order: Politics and Social Change in the USSR. By Peter H. Juviler. (New York: Free Press, 1976. Pp. xii + 274. $13.95.)

The author's objective in this short book (178 pages of text) is to describe "the Russians' quest for law and order." He begins with the great judicial reform of 1864 and, through a more or less chronological treatment that takes up about half of the book, works his way through the Soviet period to the 1970s. While pointing out the solid achievements of the 1864 reform, he also emphasizes that it, "like the Soviet ones, worked out to a compromise between new ideals, old ways, and political expediency" (p. 7). Some of the author's most useful observations involve the historical parallels he draws, such as this one, between the Russian and Soviet periods.

Juviler shows how it was Lenin's hope to create "a very simple machine" (p. 17) for administering justice in the new Soviet state. But this hope faded early and he and his lieutenants soon faced "the need for a growing apparatus of law enforcement and justice" (p. 18). In this part of the analysis Juviler is retracing familiar ground already covered by John Hazard and others. But he deals with the subject competently and provides a good survey for readers who are new to the subject. All of the great names of Soviet law of the twenties and thirties are mentioned, including Stuchka, Kurskii, Pashukanis, Krylenko, and Vyshinsky, among others. And he provides useful information on Lenin's role in law reform in the early years after the revolution, including a discussion of Lenin's ambiguous attitude toward the Cheka and his condoning of terror.

Chapter 3, on the Stalin period, is probably the most useful part of the book. Here Juviler challenges what he considers the widespread assumption that the repressive totalitarianism of the period "secured law and order" (p. 42). His conclusion is that even before the Great Terror, a breakdown of law and order was pervasive. And the figures he produces on convictions appear to support this conclusion. One may take issue with this point of view, however, and the reader should pay close attention to Juviler's line of argument. To me, his case is not conclusively made. "Law and order," to us, means basically the elimination of crime in the streets and like offenses, and Juviler seems to start out with a similar conception. But his conviction statistics appear to contain a large element of repression of economic and political "enemies" of Stalin's regime, and Juviler's conception of "law and order" seems to shift. This impression is confirmed at the end of the chapter where one reads (p. 67) that the "basic point of this chapter is that, under Stalin, new policy did not respond to crime. Policy created crime" in the sense of "widespread violations of legality and lawless repression."

Still, what is important in the chapter is the attempt to place legal developments in the broader context of political and social change. Unfortunately, providing a similar context for the discussion of legal developments has not been as well achieved in other parts of the book. The problem, it seems to me, is Juviler's failure to articulate a specific goal for each of the chapters (and for the book as a whole) and to carry through his analysis and discussion to the achievement of that goal. Other than in Chapter 3 the book falls short in this respect. One of the reasons for this may be that several of the chapters (or parts thereof) were originally published elsewhere and don't fit in well with the author's apparent intention for this book.

The latter chapters deal with legal reforms in the post-Stalin period, the revival of criminology as a discipline, the debates in recent years on the causes of crime, and the political restrictions thereon. If these chapters are somewhat out of phase with the rest of the book, they are at the same time a gold mine of information on these subjects which scholars for years to come will consult with profit. One of the most interesting aspects of this part of the book is the author's thorough job of discussing and manipulating the meager amount of published crime statistics in order to attempt more realistic judgments about the level of crime than the Soviet authorities disclose. His results are quite unsatisfactory in terms of what we can conclude about the "real" level of crime. But at least Juviler shows us, in some detail, what a difficult job it is. And he gives some hint as to the difficulties even Soviet insiders must face in measuring crime in the face of local police officials fixing crime figures to make their localities look better. If there is any problem that will deter the reader from profiting fully from this useful information, it is the sheer mass of detail that Juviler introduces into his discussions, which sometimes gets in the way of his analysis.

In spite of the shortcomings mentioned, the reader will find this an enormously informative book containing many shrewd observations on the current Soviet legal scene.

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The Economic Superpowers and the Environment: The United States, the Soviet Union, and Japan. By Donald R. Kelley, Kenneth R. Stunkel, and Richard R. Wescott. (San Francisco: W.H. Freeman, 1976. Pp. viii + 335. $11.95, cloth; $5.95, paper.)

This book provides a useful summary of important events and trends in the handling of environmental issues in three countries—the United States, the Soviet Union, and Japan. The basic theme is expressed early in the book (p. 28): "Whether capitalist, communist, or on the middle ground of a guided or regulated economy, each of the three nations has experienced a similar pattern of environmental deterioration." Why is this? Although several variables are mentioned, the incentive and evaluation systems are forwarded as playing important roles; "all three economies are characterized by an environmental insensitivity of their economic accounting or success-measuring systems" (p. 288). In all three countries "one can observe a marked dissociation between what is said and what is done."

The style of the book—with each chapter divided into three sections, one for each country—leads to a certain feeling of ennui in the reader. The text chronicles pending resource scarcity, energy shortages, and environmental deterioration. Reading about each country's environmental experiences, creates the overwhelming impression that—although the names, numbers, and currencies change—the stories are very similar.

The common elements among the three countries include: a reluctance to count pollution-control expenditures as "productive" investments, a refusal to charge scarcity prices to promote water conservation, and collaboration between industrialists and government leaders which constrains the influence on government decision makers of citizens promoting environmental protection.

This book does identify some differences among the countries. The more decentralized American system has multiple access points for environmental lobbying; obtaining recognition of particular problems is thus easier. On the other hand, in the United States governmental initiatives can often be blocked by various interest groups. The Russian and Japanese systems appear harder to move, but they may be more effective once action is begun.

I have several specific criticisms of this book. First, while the authors estimate the "damages" of pollution in different countries, the methodology for making such calculations is presented only generally. Nevertheless, the listing of specific environmental damages, if not the rigor of the damage estimates themselves, does give credence to the pervasiveness of the problem. Second, the reviewers were struck by the application of the term "militant" to such U.S. conservation organizations as the Sierra Club and Friends of the Earth. Given the range of activities of these groups, such appears to be an overly broad definition of the term. In a more important vein, the authors' nation-by-nation coverage of certain topics is disturbingly uneven. For example, the section on Japan includes a discussion of dangerous problems such as incidents at the Tsuruga nuclear power station and the siting of nuclear plants in earthquake-prone regions. Similar difficulties in the U.S. (at the Enrico Fermi power plant near Detroit and with California nuclear stations near the San Andreas Fault) are not discussed.

The treatment of international cooperation in such important areas as fisheries conservation has been quickly outdated by events. The authors discuss, and seemingly prefer, agreements for informational cooperation which have proven difficult to negotiate or implement effectively. With many of the programs for remedial action mentioned, the authors seem preoccupied with the form of the institutions rather than with the substance and environmental impact of their operation. The result is a paucity of comment on the positions of the three nations on such unilateral or independent initiatives as declarations of 200-mile offshore coastal zone lines. Such initiatives may result in more effective conservation than the proliferation of multilateral conferences often more concerned with political rhetoric and ideological harangue than with environmental protection.

In summary, the book is interesting in its presentation of a large amount of information on environmental problems in the United States, the Soviet Union, and Japan. The volume is generally sensible in its analysis and prescriptions. As noted in the preface, the study is not thoroughly integrated; "at best it may be described as a quasi-comparative treatment of the three superpowers" (p. viii). Furthermore, since the thrust of the book is mainly descriptive, some readers may be disappointed in that the discussion is seldom enlivened by reference to any models or theoretical constructs which might provide a framework for extended research on the subject.

BRIAN J. KATZ
LESLIE L. ROOS, JR.

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New States in the Modern World. Edited by Martin Kilson. (Cambridge, Mass.: Harvard University Press, 1975. Pp. 254. $15.00.)

The problems with this collection of essays begin with the title. It makes a claim the text does not live up to. As is so often the case, one must read beyond the implicit colon to find out what the book is truly about. In this instance, the regionally specific case studies are limited to the African continent, while the more general essays have little distinctive bearing on "new" states as such. The ill-focused preface contributes such sentences as: "By convulsive cleavage I mean a centrifugal conflict dynamic," but does not otherwise clarify the thematic thrust of the volume.

What does appear to hold these essays together is the origin of their distinguished authors. Nearly all were graduate students at Harvard, or now teach there, or both. The remainder are affiliated with neighboring institutions. As such, all have perforce responded to the teachings of the man to whom the book is dedicated, Rupert Emerson, who barely two decades ago was the sole political scientist at Harvard professionally engaged in the study of politics in the nonwestern three-quarters of the globe. It is the more curious that not one essay refers, even comparatively, to Southeast Asia, a region to the study of which Emerson devoted more than half his professional life and on which he became an acknowledged authority. But the Emersonian tradition is noticeable, for better and for worse, in the authors' clear preference for wisdom over science. Specific cases are cited to illustrate—occasionally to illuminate—an assertion, but never to test it systematically.

Further puzzles arise from the apparent timing of the essays' composition and publication. The lead essay by James Coleman contains no references more recent than 1966 to the politics of a continent notorious for the volatility of its regimes. Without embarrassment, Nadav Safran offers a detailed analysis of a 1962 political charter as a clue to contemporary political thought in Egypt. Joseph Nye refers to Uruguay as a model democracy. Other essays appear to have been composed nearly a decade later; and the volume as a whole is so disfigured by fractured sentences and typographical errors (Freud is said—to take one example among startlingly many—to have analyzed "neurotic quilt feelings") as to suggest a break-neck editing job more reminiscent of yesterday's *New York Times* than of the normally conscientious Harvard University Press. There is little consistency between the spelling of authors' names in the text and in the footnotes at the bottom of the same page. Such distractions do not enhance confidence in essays already vulnerable to criticism on other grounds.

For many of these essays do indeed seem open to rather elementary forms of criticism. The above-mentioned Safran essay, for example, asks readers to accept a single official government publication without argument as a valid indicator of contemporary trends in Egyptian political ideology. Though the accompanying commentary is perceptive and judicious, no effort is made to compare the charter with other official pronouncements, to correlate it with official policy or practice, or to consider its relation to unofficial streams of elite or popular thought. Leo Gross' essay on self-determination in international law belabors a single formalistic point without exploring the ambiguities arising in the attempt to determine the identity of the "people" to whom self-determination is to be given. Most political scientists are likely in any case to be primarily interested in assessing the consequences that would follow from common acceptance of the United Nations General Assembly's asserted powers in this area—a topic on which the essay remains silent. The chapter by Karl Deutsch and Dieter Senghaas on the "fragile sanity of states" avoids certain well-discussed pitfalls in transposing psychiatric concepts from the level of the individual to national collectivities, but hangs the substantive thesis without further defense upon the assertedly self-evident proposition that feedback cycles and self-correction in reality testing by individuals are uniformly more frequent, richer, and more reliable than analogous processes at the national level. Certain other essays are difficult to criticize mainly because they offer so little to criticize.

Yet if the whole to which this volume adds up is far from satisfactory, there are individual parts of much merit. The essays on African nationalism by Coleman and Crawford Young, if now a little tired and familiar, are impressive syntheses of the literature that would have made compelling reading ten years ago. Though marred by discursiveness, overlapping categories, and an unwarranted tendency to imply that a selective assembly of examples exhausts a relevant universe, Stanley Hoffmann's essay reaches some provocative conclusions regarding the costs of international stability; the Deutsch-Senghaas article falls into three weakly related parts but confirms, in its middle section, Deutsch's remarkable ability to stimulate the imagination through his transformation of everyday experience into the conceptually operational language of cybernetics. Perhaps the most wholly satisfying chapters are Joseph Nye's crisp inventory of the political factors affecting the size of African states, and a multidimensional analysis by Willard Johnson that fully accomplishes its announced objective of presenting the integration of the anglophonic and francophonic Cameroons as "a test case of the practicality of pan-Africanist aspirations." In such essays as these one sees the hope, imperfectly realized in the event, out of which this tribute to Rupert Emerson must have been conceived.

GEORGE VON DER MUHLL

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Law v. Order: Legal Process and Free Speech in Contemporary France. By Jerome B. King. (Hamden, Connecticut: Archon Books, Shoe String Press, 1975. Pp. 206. $12.50.)

American scholars, whether political scientists, historians or lawyers, have generally neglected the jurisprudence of civil liberties in foreign states, even those sharing the democratic predilections of their own. Jerome King's study of certain aspects of freedom of expression in France is a welcome addition to sparse existing literature and, in some respects, is a pioneering work.

In this short book King does not essay a comprehensive exposition of the jurisprudence of freedom of expression in France nor does he undertake a systematic comparison of French jurisprudence with that of the United States or of any other country. His undertaking is vastly more ambitious: "This study aims to depict an inherently elusive subject—the spirit of a nation. The nation is France and the aspect of her spirit I am after is civic spirit.... If one is to come to grips at all with the 'inarticulate major premises' of a country, he must do so by drawing inferences from the way its institutions work" (p. 7). The spirit identified by the author is *incivisme*, and his method is "to show how the French legal system today discovers the kind of knowledge necessary to assign legal liability" (p. 13). In this endeavor King considers the division of responsibility between the administrative and judicial court systems—the one concerned with the legality of official acts to maintain public order, and the other with the civil or criminal liability of private individuals—and he focuses upon the distinct and sometimes contradictory jurisprudence of the two jurisdictions in their respective dispositions of free-speech issues. The distinctions between private and public law in France, he argues, "correspond to a profound sense of how difficult it is in the French tradition to equate law with order, or even to take law and order as being naturally joined by the conjunction *and*" (p. 14).

Mere statement of the foregoing thesis suggests the immensity of the task and the intrinsic difficulty of executing it in an altogether persuasive manner. A major pitfall in undertakings of this nature is that the writer simply reads into evidence whatever conclusions he seeks to infer, but this reviewer believes that King has succeeded in avoiding this pitfall.

Still, there are problems in execution. Perhaps one may put to one side the exceedingly eclectic nature of the materials analyzed. However, approximately twenty cases from two distinct jurisdictions is at best a small sampling, and the fact that King includes no case decided after 1963 is troublesome. The rich body of recent secondary works on *les libertés publiques* by French scholars does not surface in this study. The major studies of Braud, Dran, Bretton and Heymann, to name only a few, might provide different perspectives on the jurisprudence which is considered here.

Of greater importance are substantive omissions which truncate and distort the institutional framework from which King makes inferences about the spirit of the nation. He does not consider the French theory of separation of powers, a version having vastly different institutional consequences (especially for the role of the judiciary) from those of the American version, although this theory, as an operationalized response to a traditional distrust of the law courts dating from the *ancien régime*, may very well constitute the unidentified "imperative of the culture" (p. 14), which accounts for the perdurability of the dual court system. Moreover, the absence of judicial review (by either the ordinary or the administrative courts) of the constitutionality of statutes, while not warranting the overemphasis accorded it by those American scholars who equate the rule of law with their own version of judicial review, is a salient feature of the institutional framework having consequences not only for what French courts do not decide, but also for the products of what they do decide. Inexplicably, the author gives no real attention to this matter. Of a more technical nature, but nevertheless important in fleshing out the institutional aspects, are such matters as the available remedies. In Anglo-American law the availability of preventative relief does much to shape the jurisprudence of the courts and has significant consequences for the substance of the rights asserted. In French law the *sursis d'exécution* with reference to official action operates only in the administrative jurisprudence, and there within an exceedingly narrow but slowly expanding compass, with consequences of a very different kind.

Quite possibly none of the foregoing questions, if supplied and considered, would alter the conclusions reached by King. But is it not possible that the jurisprudence of freedom of expression in France in its manifestation of *incivisme* reflects less the spirit of the nation than the peculiar institutional limitations under which its courts operate? The limitations themselves may, of course, reveal the spirit; but those limitations must be identified and acknowledged in order to sustain such a thesis.

Despite my reservations, however, I find this an extraordinarily provocative book. Within the limits of its premises it is subtly and closely reasoned, and written against a background of general humanistic learning.

CLYDE E. JACOBS

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The Comparative Study of Political Parties. By Kay Lawson. (New York: St. Martin's, 1976. Pp. xiii + 261. $12.95, cloth; $5.95, paper.)

Kay Lawson does not confine political comparison to brief time periods and carefully circumscribed boundaries. To her, comparative work holds promise and excitement precisely by "daring to overstep geographical, cultural, and historical boundaries, searching out both similarities and differences in the ways of resolving, or failing to resolve, political problems" (p. vii). Students in comparative government courses should find this slim volume a thought-provoking combination of largely unfamiliar case materials and conceptual discussions that clarify the processes and deepen understanding of how three particular political parties have prospered and changed in recent decades: the Democrats in America's dual-party system, the Gaullists in France's multiparty setting, and Touré's ruling PDG in socialist Guinea.

After an inventory of how parties are studied, Lawson fashions a common-sense agenda, unabashedly eclectic. Three parties in three nations on three continents will surely provide rich empirical material. Five modes of analysis will be used: historical, structural, behavioral, functional-systemic, and ideological. The study of *particular* parties has been neglected too long because political scientists tend to theorize only about "party systems." By making a multifaceted comparison, point by point, of three distinctive parties, each embedded in a strikingly different political milieu, Lawson aims to clarify how each party actually shapes the representation of citizen interests when public policies are being made and carried out. "The perspective is not on what the party does or does not do for a regime's stability, but rather on what it does or does not do for the effective representation of the citizen" (p. 234). What kind of political linkage between certain publics and certain policy makers does each party provide?

This book employs a sensible but difficult method of inquiry. It is not substantially based on primary source documents or field investigation. The case studies are exploratory rather than definitive. They are not used to enliven the presentation of quantitative findings, nor do they demonstrate how a rigorous conceptual model of parties could be applied in different settings. Secondary sources are used judiciously, although some work is missing that would have complemented and complicated what she presents (e.g., Duncan MacRae or Nathan Leites on contemporary France). For Guinea, she relies on specialists, most of whom have written in French—Claude Rivière, Bernard Charles, Henry de Decker—and have not previously had their views set forth in English. She also relies much on Sékou Touré himself, and on government releases, and probably it is unavoidable that there is much reading between the lines of public speeches or official statements. For the American case, Lawson draws selectively on the voluminous professional literature about electoral behavior and party organizational processes, as well as the macro-political analyses of Key, Ladd, Scammon and Wattenberg, Phillips, and others. Lawson is not uncritical of what she uses; neither does she often take time to spell out her reservations.

Lawson's analytical scheme enables her to be insistently comparative. She examines six aspects of party: origins, organization, members, leaders, issue formulation, and regime and system roles. She discusses what is known about each and formulates a set of key questions the answers to which she then explores in depth as they are found in the French, Guinean, and American cases. In effect, she develops her three case studies simultaneously. By moving back and forth between explicit theoretical considerations and stubborn empirical realities, she gradually constructs a jigsaw picture of each party which *does* clarify the various ways—biased, attenuated, truncated, even accidental in part—in which they are linkage mechanisms.

Before each case study, a brief overview of each country's political history is provided. Some forces are transforming the political order, some are stabilizing its character. Necessarily the time perspective is foreshortened for Guinea, extending only a few years back into the colonial period: even before breaking from France in the late 1950s, Sékou Touré dominated Guinea, and his autocratic regime remains in power, but "once Toure's hold on the party begins to weaken, issue-based as well as power-seeking factionalism will emerge rapidly in the PDG" (p. 151). Similarly, De Gaulle's organized following is examined mainly through wartime and postwar developments, the Algerian crisis and the Fifth Republic. The distinctive features of French political culture are changing: "It just may be that the twentieth century, with its demands for *mobilisme*, has arrived in France" (p. 37). Again, the American case study deals with post-Kennedy developments, preceded by a sketch of the nation's political history seen as a "delicately evolving balance between the forces of centralization and democratization," periodically ruptured by the political divisiveness of explosive issues—like civil rights at home and entanglement in Southeast Asia.

Lawson's discussion of grand systemic transformations provides a backdrop. Her attention turns to particularities of party history, organizational forms, participants, issue concerns and accomplishments. When parties seek to get and keep parliamentary status, quite different patterns form than when an underground movement pursues subversive goals or when splinter parties break away on doctrinal grounds. Crises typologies are directly in point for all three case studies: not mentioned, surprisingly, is the cleavage schema of Lipset and Rokkan for tracing the genesis of European party systems.

Consider Lawson's distinction between the locus of power and its effective concentration. In concentrated cases, where a party is centralized, it may still manifest cadre, vanguard, or mass forms of organization. If power is dispersed, the nuclear unit may be a committee, a club, or a convocation "in which oligarchical decision making is the norm but local members are occasionally summoned to pitch in and help" (p. 78). The typology fits her cases poorly, but, as she rightly insists, the effort at structural classification clarifies how different forms shape daily intramural power relationships.

Leaders and activists are dealt with in terms of their credentials and their viewpoints. Various environmental factors affect leadership selection. But internal party practices shape recruitment also (e.g., top-side or grassroots control of nominations, combat readiness and utility of the party as sponsor). Distinctive features of each party emerge. Surprisingly, neither apprenticeship practices nor political opportunity structures are discussed. However, the open latticework of Lawson's key-question approach invites one to think of yet additional conceptual tools that might be helpful.

Lawson examines issue formulation—how concerned parties are with issues, what ideological bias persists, what factional divisions emerge, what communication methods effectively reach the public. She looks also at their governing roles—their programs, if any, the breakdowns of party discipline, the pressures from non-party sources, and the difficulties in controlling those public functionaries and private-sector elites who make policy in different fields. Finally, she considers their integrative functions—providing citizens with organized alternatives, curbing the divisive impulses among the active, and sustaining the credibility of non-governing parties so they are seen as organizations significantly affecting the nation's performance. Fascinating problems arise in fitting real cases to these considerations.

Two rather different ways can be used to test ideas—by paying rigorous attention to their logical development, and by trying to fit them to empirical cases. Kay Lawson in this short volume has done the latter, taking as given what are widely recognized as significant conceptual distinctions in the study of party and examining how poorly or well they work when applied to contrasting cases. It is a good step in the right direction.

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Economic Planning and Politics in Britain, 1945–1974. By Jacques Leruez. Translated by Martin Harrison. (New York: Barnes and Noble, 1976. Pp. vii + 317. $20.00.)

A comment on the second page of this book almost says it all. Writing in the preface, Jack Hayward of the University of Hull remarks: "Given that references to the abandonment of planning make little sense when once it is conceded that planning never seriously got started. . . ." *Economic Planning and Politics in Britain* tells the story, first, of how planning never got started in the late 1940s and then how, through a series of mistakes, misfortunes and political frauds, economic planning was mangled by successive governments between 1960 and 1974.

Jacques Leruez is a rarity on several counts. He is a political scientist with a serious interest in economic issues, government institutions, and politics. His book is a welcome addition to a vital and still much neglected field in political science—the politics of economic policy making. Moreover, the author is a Frenchman with a deep understanding of the British political idiom and no more than an appropriate sense of condescension in comparing the British and French planning experiences.

The strength of this book is its ability both to describe the detailed political-bureaucratic moves and to set out the relevant economic record. In doing so, the author manages to unravel a good many intricate threads to suggest how British economic developments were, at the same time, the creation and the cause of these governmental moves. To anyone with a serious interest in down-to-earth political economy and in what the British political and economic elite has been doing since the war, this book is well worth reading.

Unfortunately, there are also some outstanding weaknesses. One's misgivings begin with a table of contents containing 17 chapters in a 300-page book. The basic material is informative, there are interesting sections, but, alas, the book simply does not hang together. Leruez fails to extract any themes or other points of analysis that would integrate his material. This becomes evident in the six-page final chapter entitled "Conclusions." Since it is not clear what questions the author was trying to answer, it is difficult to find anything upon which to hang conclusions, either of a retrospective or prospective nature. Instead, the book ends with a fairly ambivalent recitation of issues that must have seemed topical to the author as he was finishing this edition in 1974—e.g., the questioning of economic growth as a policy aim, the quality of life, oil supplies, the energy crisis. Important? Obviously, but one would have thought any review of 30 years of economic policy making might have provided something more with which to work. In the end, very little of what the author has to say in conclusion grows out of the substance of his book.

Thus, anyone who wants to read something with a coherent intellectual thrust on the economic issues will do better to try and keep up with Samuel Brittain's writings. Anyone wanting a cogent political interpretation of these developments will have to rely on Samuel Beer and hope for a new edition of *British Politics in the Collectivist Age.* Any anyone so bold as to hope for something explicitly linking the two Sams—i.e., the politics and policy of British economic life—may have to wait for the new edition of Shonfield's *Modern Capitalism* (which is now nearing completion). The virtue of Leruez's book is its description of who did what when; its failing is that it could have been so much more.

Contained within *Economic Planning and Politics in Britain* are some useful lessons for the U.S., particularly at a time when Washington is under the influence of the first president since Herbert Hoover with a penchant for planning and engineering. These lessons are not answers to simple-minded questions (such as whether planning does or does not work). Instead they are the cautions of political common sense.

One lesson is that any formalized attempt at "planning" tends to arouse a debate that distracts attention from real issues. The planning process itself becomes the point of contention; lost from view are the hard policy choices that exist regardless of whether or not they are formalized in a proper planning exercise. This lesson, in effect, reverses the normal contention by advocates that planning is necessary to bring policy options and priorities into the open. In practice, debate on planning as a process displaces choices about policy substance.

A second lesson is that planning places considerable strains on political resources. If the planning (in whatever sense that term is used) is to be real, politicians have to be willing to have planners saying things—including unpleasant things—about the facts as they see them. Even worse, politicians have to mean what they say in their plans or suffer an enduring decline in credibility. Worse still, politicians have to stand behind their plan, bypassing any short-term advantages of abandonment and antagonizing people who expect the normal repetitive bargaining process to prevail at all times. Unfortunately, the need for planning is likely to be greatest when (apart from war) the political resources to carry it out are smallest.

There is perhaps another lesson. Leruez's book might be subtitled "how the rot set in," and the 1960s were clearly vintage years for rot. These were also good years for a series of government gestures promising new initiatives, reorganizations, planning exercises, and sacrificial efforts. The fact is that political rhetoric about sacrifice, crisis, a rebirth of commitment and all the rest usually varies inversely with politicians' willingness to tell people the bad news. Despite a trail of symbolic gestures, the politics of British economic planning is a story in which no one accepted the responsibility for saying, quite specifically, what things should be done and then trying to do them. That has little to do with planning and everything to do with political leadership.

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Politics in Liberia: The Conservative Road to Development. By Martin Lowenkopf. (Stanford, Calif.: Hoover Institution Press, 1976. Pp. viii + 237. $9.95.)

In the continuing ideological debate regarding the best course for African development, attention has been focused on a relatively few experiments in agrarian socialism (Tanzania and Guinea) versus several models of externally supported forms of state capitalism (Kenya and the Ivory Coast). A less frequently cited example of the latter is Liberia, which since 1950 has attained a per capita increase in GNP surpassed only by the oil shiekhdoms. Growth has been based largely on American and European exploitation of its high-grade iron ore. With the exception of Elliot Berg, however, most serious students of Liberian development tend to be highly critical of the expanding economic, political, and social chasm between the coastal Americo-Liberians and the tribal majority of the interior. Berg, unfortunately, has taken the more comfortable role of the critics' critic rather than presenting his own documented apologia. Martin Lowenkopf attempts to do what Berg has failed to do.

Lowenkopf argues that Liberia is an example of the "conservative" route to modernization "characterized by the adaptation of traditional institutions to the functions of a modern state, and the maintenance of the bonds of the antecedent social and political order during the transition to a modern one" (p. 8). Liberia, Lowenkopf insists, is undergoing significant structural and institutional changes without resorting—as others have—to the cataclysmic mobilization of the entire society upon the ashes of the preexisting social system. His conclusions are based upon intermittent field research conducted between 1964 and 1971, and he draws heavily upon both Liberian and expatriate sources in providing substantial documentation of the Liberian economic "miracle."

Although Lowenkopf's analysis makes frequent oblique references to the works of Huntington, Deutsch, Duverger, and others, there is no single over-arching theory to which Lowenkopf subscribes in organizing his case. My more major concern, however, is whether the author has in fact provided the neutral analysis he claims for himself. Taking pains to set his study apart from the more negative conclusions drawn by Clower and Dalton in *Growth without Development* (1966) or my own study, *Liberia: The Evolution of Privilege* (1969), Lowenkopf nevertheless pursues the very questions raised in these more critical studies. Indeed, he provides additional data and an extended time perspective which implicitly constitutes a harsher indictment of Liberian development than that provided by Clower-Dalton or myself. His optimistic conclusions do not square with his pessimistic findings. I refer, for example, to the extraordinary data which is sandwiched in between his optimistic conclusion on page 60 regarding the extent of development and the betterment of the material condition of most Liberians and his statement on page 82 that "Liberian economic development appears not to have placed the fate of its people in foreign hands, or to have irrevocably locked them into a rigid class structure." The intervening data catalogs Liberia's overwhelming reliance upon external capital and skilled workers; the absence of significant Liberian involvement in economic decision making; the radically unbalanced nature of economic growth; luxury spending by the elite; the inefficient and corrupt spending of public revenues; the confiscation of vast tracts of tribal land by Americo-Liberian officials for personal gain; the severe and recurrent international balance of payments problems; and the mixed results of economic growth insofar as the tribal masses are concerned. In later sections, as Lowenkopf examines the True Whig Party, the military, and other structures, the impression is reinforced that the gap between the coastal elite and tribal masses is widening.

Lowenkopf's suggestion that expansion in scale of the economy, government, and educational structures will lead to both greater participation and a more equitable distribution for the tribal masses is more an article of faith rather than a hard weighing of the evidence. While others—including myself—have suggested that the need for fiscal efficiency and technological competence may ultimately compel the social and political structures to change, we advanced that as a *possibility* and not as an *inevitability*. The contemporary world has far too many examples of societies which move—as Huntington suggests—to greater rationality and greater specialization, without the corollary alteration in participation patterns. Lowenkopf himself recognizes (p. 138) that the expansion of government in Liberia has largely increased the scope and character of patronage available to the entrenched elite, and that the educational system which "aims only at producing cadres of technically and administratively competent workers may also create dispirited, rootless, rebellious students" (p. 141). Indeed, given the general situation in Liberia, it is as safe to predict violence as it is to predict that moderation will characterize the move to modernity.

Lowenkopf's analysis clearly minimizes the remarkable durability and adaptability of the Americo-Liberian family system, which has served the group well during more than 150 years of external and internal threats to the group's survival. New forms of selective family reckoning, alliances forged with the more promising of the educated tribal youth, client relationships established between Monrovia-based lawyers and their newly educated tribal "wards," and other factors have enhanced the political solidarity of the Americo-Liberian elite. Equally significant are the new forms of alliance created between the traditional chiefs and the land-hungry political leaders who are creating huge estates in the interior. Indeed, this is a situation to which Lowenkopf devotes only a scant few pages and somewhat curiously suggests that "the manner in which land is obtained secures some respect for tribal prerogatives" (p. 78). Finally, given the history of trade unionism is Liberia and Africa in general, I see little justification for Lowenkopf's optimism regarding labor unions as a possible source of political change.

J. GUS LIEBENOW

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Changing Campaign Techniques: Elections and Values in Contemporary Democracies. Edited by Louis Maisel. (Beverly Hills, Calif.: Sage, 1976. Pp. 274. $17.50, cloth; $7.50, paper.)

Editors of scholarly yearbooks apparently have something in common with many novelists, i.e., they find it difficult with their second works to match the quality or success of their first efforts. *Changing Campaign Techniques* is the second volume in the Sage Electoral Studies Yearbooks, a series promoted by its publisher as offering original essays, possessing cross-national and interdisciplinary perspectives, on a central theme each year. Volume 1 in the series, co-edited by Louis Maisel, largely lived up to the publisher's promise. At least each of its essays did pertain to *The Future of Political Parties* (Beverly Hills, Calif., Sage, 1975). By comparison, Volume 2 is a strange and disappointing potpourri loosely organized, according to Maisel, around the task of examining "the effects of changing campaign techniques" and "the consequences for democratic elections." Seemingly motivated by concerns over whether or not candidates really can be packaged, sold, and bought like laundry detergents, this collection of essays is supposed to tell us "if what have changed are the techniques but not the functions of elections" (p. 11).

Certainly to answer that question would be to make a contribution. Unfortunately the essays in this volume not only fail to answer the question, most don't even speak to it. One reason is that the "changing campaign techniques" appears in two senses; some authors of the volume's essays use one, others employ the concept differently. In one instance changing techniques refer to the evolving technology of campaigns—television campaigning, strategic planning of party organizations, increasing sophistication in allocating campaign resources, and public opinion polls as information sources. In a quite different sense several authors equate "changing" with "reform," and the focus of essays is on how best to make elections more democratic and responsive. Taken as a whole, the volume adds little to our understanding of changing campaign techniques in either sense. Some of the essays offering a cross-national flavor seem included more out of convenience than any relevance they have for assessing technology or advocating reform. The yearbook is neither comprehensive (the editor explains the omission of a full discussion of the effects of polls, for instance) nor current (thus, essays touching upon television's role ignore the published studies of either Mendelsohn and O'Keefe or Patterson and McClure concerning the 1972 presidential election).

Although it is difficult to give the volume high marks—even compared with its predecessor—as an integrated effort accomplishing the stated purposes of the annual series of Sage Electoral Studies, it contains a few chapters that merit reading. There are three general categories of essays. The first deals with proposals for reform, particularly public financing of U.S. elections. Joel L. Fleishman presents a persuasive case for public financing, Jewel Bellush and William J. D. Boyd present a persuasive case against, and Maisel weighs the potential impact of public financing upon the development of minor parties. All three essays were written before the 1976 presidential election and amendments to the Federal Election Campaign Act made that year. Nonetheless the pros and cons of public financing have changed little and the three essays provide useful summaries that could appropriately appear in a debater's handbook.

Three of the volume's chapters address the question of effects of campaign technology. The two best deal with the consequences of political techniques in British general elections. Anthony Smith describes how the forms and methods of electoral television (especially in combating the impending boredom of the audience) shape the abilities of the electorate to perceive events in Britain's "advanced telocracy." Drawing upon studies of the 1974 British general elections, E. J. Dionne argues that new campaign technology—especially television—has not changed British politics greatly, that there have been more continuities than changes, and that nonmedia and nontechnological factors explain a significant number of those changes as have taken place. The remaining essay that touches upon the volume's stated purpose offers a compilation of the factors that enter into a campaigner's allocation of electoral resources: using data from the 1972 U.S. congressional elections, Paul A. Dawson and James E. Zinser describe an instructive test of their model.

The book's remaining three chapters, when they deal with campaigns at all, describe long-range effects under diverse circumstances. Alastair Davidson accounts for the growth in the viability of the Italian Communist Party by describing shifts in party strategy; Victor J. Hanby looks at the resurgence of the Scottish National Party; and Alfred Grosser focuses upon French politics to speculate about the relationship between responsibilities governments have to inform and to be informed—through polls, television, and the printed media.

In sum, readers interested in pondering the effects of changing campaign techniques will find relatively few of the chapters in this volume helpful. If they are of a mind, however, they can uncover some useful thoughts about a variety of topics and electoral systems. These may be jotted down in the six blank pages inexplicably alloted for "notes" at the end of the book.

DAN NIMMO

University of Tennessee

Electoral Mobilization and Public Opinion: The Venezuelan Campaign of 1973. By John D. Martz and Enrique A. Baloyra. (Chapel Hill: University of North Carolina Press, 1976. Pp. xlvi + 339. $17.95.)

John Martz and Enrique Baloyra have produced an exhaustive analysis of the 1973 Venezuelan election campaign, relying upon survey research, electoral data, and a great deal of direct observation by the authors. The description and analysis is successfully placed within the broader context of recent Venezuelan politics, and less successfully, within a general framework for comparative campaign analysis.

The authors begin with a brief but cogent review of what theoretical literature there is on the subject of campaigns, noting their intention to focus on the behavior of "campaigners" rather than the behavior of voters, as political scientists have tended to do. They present what they variously term an "analytic" or "logical" framework of democratic campaigning, which includes three broad rubrics: the political and institutional environment, the mobilization of forces, and the processes of implementation. Together they regard the environmental, mobilizational, and implementational variables as a "campaign system." After elaborating on the framework in considerable detail, the authors present a formal inventory of definitions, hypotheses, and propositions consistent with their framework, and then proceed to organize their subsequent analysis along those lines. The topics they discuss are richly detailed, particularly electoral laws and demography, candidate selection, party campaign organization, strategies and tactics, the almost daily actions of the principal presidential contestants, and the process of voting and vote tabulation. We are told, for example, that in an election where nearly 97 percent of those registered voted, the average time spent waiting in line to vote in Caracas was from two to four hours.

Clearly it is the authors' intention to rescue campaign analysis from the labyrinths of historicism and journalism, and to stress those dimensions which may be more universal, and ultimately, comparative. The use of some framework, therefore, becomes inescapable if what they do is to be compared with other campaigns in other contexts. But while they realistically caution the reader about the tentativeness and limitations of their framework, they slavishly follow it throughout the book, causing periodic interruptions and redundancies in the text. The kinds of data which the authors must employ simply do not fit as formal a framework as they try to impose. And while the analysis does stress "campaigners"—especially candidates and party leaders—some of the most interesting data and observations concern voters, particularly the survey research data. What this suggests, perhaps, is that ultimately it may be unwise conceptually, and impossible analytically, to isolate campaigners from voters in such an endeavor.

In the context of the total enterprise, these are minor flaws. This is, in a sense, a pioneering effort, particularly for students of Latin American politics, for whom the book is likely to become a standard source. Although the authors present enough background on Venezuelan party politics and institutions for anyone unfamiliar with them to make sense of the campaign analysis, I suspect most would find it difficult to retain and digest the background material presented. For those who care about such things, the book is uncommonly well written, although the publishes unfortunately has chosen to offset it from standard typewriter print.

RONALD H. MCDONALD

Syracuse University

Second City Politics: Democratic Processes and Decision-Making in Birmingham. By Kenneth Newton. (Oxford, England: Clarendon Press, 1976. Pp. xi + 270. £4.95, $11.00, paper.)

Kenneth Newton has written an excellent book on the political relationships among groups, parties, council members, and borough officers in England's second city. In framing its problems, the study, by drawing on such works as those by Michels, Schumpeter, Dahl, Key, Eulau, and Pitkin, takes as its point of departure the core of relevant political science literature. And it uses familiar survey techniques to gather systematic data from interest group leaders, council members, and party officers on both communication processes and the norms governing representational roles. As a result, Newton presents a comprehensive statement concerning local English politics that provides a standard against which future research in this area will most certainly be compared.

Within the group-party-representative framework, Newton begins with what is, from an American perspective, the most distinctive characteristic of local English politics: the dramatic impact of national political trends on borough council electoral outcomes. Newton reports that "the correlation between swings to the Conservative Party in Birmingham and in a sample of English towns and cities" for the period 1945 to 1965 was no less than .95 (p. 14). Consequently, little that is done by local council members has much impact on the local party's collective fate in local elections. And local politicians recognize this fact. Although councilors believe local issues and personalities have some electoral impact, Newton's interviews reveal that in Birmingham a majority of them—and 80 percent of the Labourites—recognize that national factors are paramount. Since according to modern democratic theory the electoral accountability that stems from two-party competition for power has been a central mechanism for maintaining democratic or at least polyarchical processes, the extent to which the electoral isolation of representatives affects their relationships with citizens, groups, and party rank and file is of central concern in what follows.

Appropriately enough, Newton turns directly to group activities themselves. In the most innovative part of the research design, he locates what is believed to be the universe of 4,250 formally organized interest groups in the city, including such apparently nonpolitical types of groups as sports, church, and youth organizations. Although subsequent contacts revealed still another thousand groups, the 4,250 provide the list from which a not altogether perfectly random sample of 576 was drawn, 466 of which responded to mailed questionnaires. The secretaries of the 70 most politically active sample organizations and a subsample of 84 of the remainder were then interviewed. From this data base Newton discerns that the most politically active groups (at the local level) are city-wide organizations (rather than those either neighborhood or regional in scope), are larger, have more income, have a paid staff, and are more politically "established," as indicated by such things as inclusion of a councilor among their group members. And these resources are cumulative, not dispersed. These are not surprising findings, to be sure, but they are still evidence using novel techniques that confirm Schattschneider's adage about the accent of the heavenly chorus in the pluralist utopia.

Newton next stresses the potentially significant role that party organizations can play in the policy-making process, and his findings bear closely on the debate between Robert MacKenzie and Samuel Beer. Although Newton does not cite Beer's response to MacKenzie's contention that the Labour and Conservative parties have similar internal structures, Beer's side of the argument is strongly confirmed by Newton's research. As compared to Conservatives, Labour councilors are more likely to cite their party organizations as a public opinion source they rely upon; are more likely to report more policy discussions at ward meetings; act in a formal context where party rules give the organization more authority vis à vis its representatives; and work in a milieu whose oral history includes many more anecdotes about party-group conflicts. Indeed, so many significant party differences concerning representational norms and practices—and not only with respect to internal party relationships—are reported that at times one wonders why Newton did not make the competing political cultures of the Labour and Conservative Party a central organizing theme of the study.

There is much more of value in remaining chapters. The familiar distinction between delegate, politico, and trustee is drawn, and reported norms—which vary across parties—are found to correspond to a variety of reported activities. The netherworld of councilor-officer relationships is examined with much intelligence, but about as little information is given as in most studies. In the end Newton comes to accept councilor claims that the officers do *not* get "too big for their boots" (p. 159). The last substantive chapter consists of case studies of the politics of housing, comprehensive education and race relations. They add a good deal of contextual material which one wishes could somehow have been incorporated into the analytical portions of the study.

On the central issue which motivates the study—democratic processes in Birmingham—Newton does not provide any clear resolution. On the one side, he finds a systemic bias favorable to the better-organized, the more established, and culturally acceptable, the most obvious losers being Birmingham's large and growing immigrant population. On the other side, he does not find the lack of electoral accountability an insurmountable obstacle to group influence and representative responsiveness. And it is only on this second point that I find myself in disagreement. The data Newton describes seem to me to depict a closed decision-making process, dominated by committee meetings, council group sessions, and contacts with corporation officers rather than a process where citizen involvement and group competition play a significant role. Although no exact comparisons with other cities in other countries are available, I suspect that the process is particularly closed in the English context because electoral incentives to respond to outsiders are lacking. But it is a pleasure to read a book on English local government so intelligently and clearly addressed to significant issues that one finds it worthwhile to disagree.

PAUL E. PETERSON

University of Chicago

Soldiers in Politics: Military Coups and Governments. By Eric A. Nordlinger. (Englewood Cliffs, N.J.: Prentice-Hall, 1977. Pp. xiii + 224. $8.95, paper.)

Soldiers in Politics, a volume in *The Contemporary Comparative Politics* series from Prentice-Hall, introduces students to a wide literature on the military in politics and it covers almost all the major issues that have been salient in the study of military organizations, coups, and government performance by military regimes in nonwestern countries. Nordlinger discusses reasons for military intervention, stressing the militaries' corporate interests but also paying attention to class and communal ties and to officers' attitudes toward politics. He evaluates how military men run governments and the effects they have on various economic, social and political outcomes. He leaves out only a few topics. He does not deal with the impact of international relations on transformations of military organizations nor does he analyze the diffusion effects across systems. He takes for granted the overwhelming importance of officers in non-western militaries and thus he does not examine relationships between enlisted men, noncommissioned officers, and officers. More attention ought to be paid to these relationships in systems where military chain of command and hierarchy cannot be taken for granted. Nordlinger's analysis, however, is comprehensive and will interest readers who know the field while at the same time introducing students to concepts and materials with which they may not be familiar.

In his 1970 article in the *American Political Science Review*, "Soldiers in Mufti: The Impact of Military Rule Upon Economic and Social Change in Non-Western States," Nordlinger outlined an argument which remains the core of this book. The argument related military behaviors to both institutional features of armed forces and to social forces, especially classes and communal groups, and elaborated on the idea of praetorian societies, an idea which owed much to Huntington's work.

Nordlinger was then, and remains now, negative about the military's impact on politics. He argues that while there are obvious differences within his two ruling types of guardians and rulers, military regimes have less variation than civilian ones and they also perform consistently worse in terms of progressive economic change (p. 176). Indeed, Nordlinger gives militaries poorer marks than civilians on a range of performance indicators, especially stability and ability to handle communal conflict.

I happen to agree with most of Nordlinger's "rough generalizations" but I have some criticism as to how he gets to them. He does not use much of the recent cross-national aggregate data work, most of which criticizes his 1970 article for using poor data and overstating his case. Recent studies have argued that on economic performance grounds, there is as much diversity within militaries as within civilian regimes and they maintain that few strong generalizations can be supported which distinguish between military and civilian economic performances. Nordlinger should have used more recent data and not relied so heavily on his own (now dated) reanalysis of Adelman and Morris' data in *Society, Politics, and Economic Development*.

In arguing his case for negative political and economic effects of military rule, Nordlinger puts his finger on the important and often overlooked inability of the military to create networks which penetrate the grass roots. He gives some good reasons why this is the case: the military's unwillingness to bargain and negotiate with groups and interests, faith in technical solutions, reliance on civil servants. He also stresses the importance of officers' attitudes towards politics and politicians. Indeed, throughout this book, Nordlinger writes about attitudes of praetorians. He argues that they have biases against mass political activity (p. 21); they share norms with civil servants (pp. 43, 121); they place high values on political order (p. 54). How does Nordlinger arrive at an understanding of officers' attitudes? Mostly, he derives attitudes from a presumed identity of officers' interests with middle classes and from organizational features which he believes inhere in armed forces organizations. He relies on some case studies where the investigators have had contact with officers and on a few officers' memoirs. But the fact is we have almost no reliable survey work on officers' attitudes. It is not that Nordlinger is wrong in ascribing various attitudes to them. However, he should be more careful in allowing for varieties of sytle and for the scope for idiosyncratic personalities to come into play in hierarchical organizations. Since he points out that officer corps deviate from ideal types which postulate their being above politics, above factionalism, devoid of communal strivings, he should have allowed also for a variety of attitudes held. Some officers have been quite willing to risk disorder, even to provoke it, for the sake of a radical restructuring of their societies. We see this, albeit in different ways, in Ethiopia and Uganda. Nordlinger also frequently refers to a loss of legitimacy by civilian regimes facilitating coups and he notes the failure of military regimes to build legitimacy. Maybe so. But again, we have little hard information regarding the legitimacy of regimes in developing countries or variation in a regime's legitimacy over time or between regimes. We cannot deduce legitimacy from longevity or deduce absence of legitimacy from the occurrence of coups.

Nordlinger tries to isolate various characteristics of militaries to see their consequences for governmental performance. This works better for political than economic consequences. The best predictor of economic performance would appear to be previous economic levels and previous economic performance, not regime type. Indeed, the strength of this book is not its use of aggregate data to make generalizations through quantitative analyses. Rather, it lies in

Nordlinger's ability to use a number of case studies and general treatments of military rule and coups and to make his own argument coherent and clear. He isolates critical issues in the debate on military rule. The book's weaknesses point the way to the need for more case studies on civilian-military relations, for better analyses of armed forces organizations themselves, and for continued collection of time-series data and more careful use of data.

HENRY BIENEN

Princeton University

Social Democracy and Industrial Militancy: The Labour Party, the Trade Unions and Incomes Policy 1945–1974. By Leo Panitch. (Cambridge and New York: Cambridge University Press, 1976. Pp. 318. $22.50.)

Leo Panitch's central argument in this study is that the failure of British trade unionism to convert industrial militancy into revolutionary political consciousness is due to the union movement's ties to what is an "integrative" Labour party. Panitch points out that Labour's economic and industrial policies have stemmed much less from socialist planning than from its integrative ideology which emphasizes above all the creation and maintenance of compromise between the classes in British society. Labour leaders would certainly prefer a more egalitarian distribution of income and wealth and less reliance on promoting the needs of the business community. But their commitment to "nation-class synthesis" produces policies which "accord with the inner dynamic of such a society—the profit motive—to the enhancement of the class from which this dynamic stems and to the detriment of the working class. The new social contract in this context is a contract not only between unequals but one in which the guarantor of the contract—the state, even under a Labour Government—is not and cannot be disinterested and neutral between the classes" (p. 245).

Labour government incomes policies, proposals like "In Place of Strife" and other demands for trade union cooperation in the government-defined national interest should therefore not surprise us. Nor should we be surprised at the tensions and contradictions that these demands impose on the relations between the two wings of the Labour movement. But why do the unions not opt out of the relationship, perhaps to found a new party? Answering this important question, Panitch asserts that while such a break is not impossible, the glue of union loyalty to the Labour party over so long a period has become remarkably fixed and that the continuous fear of Conservative attack helps to keep it so.

Given the small likelihood then of an all-out break, what are the alternative possibilities for a "remobilization" of Labour from within? "Certainly the magnitude of the task is obvious—the whole history of the Party has to be overcome" (p. 258). Yet, Labour can continue its integrative role only at the price of intensifying the "... strains and conflicts which the Party's ties to the organized working class have repeatedly produced" (p. 258).

Panitch's study concludes in 1974 before Labour returned to power. We therefore now have benefit of several more years of evidence about the relationship between the Labour party in office and the trade union movement. Obviously, there has been neither a rupture in the relationship nor a remobilization of the party. There has been instead a tenuous accommodation. The trade union movement has enjoyed unprecedented access and influence on policy in nearly every area of government activity and to boot has had the TUC-designed Employment Protection Act legislated. At the same time, the Labour government has gained an unusual measure of cooperation for wage restraint, helped by overwhelming union fears that recession with very high unemployment would otherwise grow worse. Both sides therefore seem to feel, using Panitch's frame of reference, that accommodation is the best part of a bad deal—at least for the moment.

The major portion of the book deals with the period of the Wilson Labour government, 1964–1970. While there are also relatively brief chapters dealing with the earlier periods and for 1970–1974, the author clearly spent his major effort on the six difficult and tumultuous years of conflict between the unions and an ever-demanding and disappointing Labour government. Panitch's work here is impressive. He has had access both to TUC Economic and General Council minutes which are indeed hard to come by. Moreover, he has done scores of interviews with a cross section of senior and junior trade union officials, politicians and others who have been closely involved in the events of this period. His other more public sources are also thoroughly and carefully done so that the pictures he draws for us are meticulously accurate. The only significant criticism of these important chapters is that the description occasionally becomes confusing because of Panitch's recitation of every detail. But in sum the work is quite excellent and should provide scholars with a much clearer picture of this period than anyone has done up until now.

GERALD A. DORFMAN

Iowa State University

Asia's New Giant: How the Japanese Economy Works. Edited by Hugh Patrick and Henry Rosovsky. (Washington, D.C.: The Brookings Institution, 1976. Pp. ix + 943. $19.95, cloth; $10.95, paper.)

When the Brookings Institution (one always wants to call it "prestigious") set out to analyze the Japanese economy, not surprisingly it first picked the most established specialists (a Yale professor and a Harvard dean) as directors, got together a team of truly big-name American economists, and spent substantial amounts of Andrew W. Mellon Foundation money on seminars and travel. More surprising is that the result of all this is a remarkably good book. *Asia's New Giant* is itself a giant (13 long chapters) and is as original and interesting as it is detailed and comprehensive. Its intention is to discover "how the economy works," particularly the causes of Japan's amazing postwar economic expansion, by examining such aspects as fiscal and monetary policy, planning, banking and finance, the tax system, international economic relations, industrial organization, technological innovation and diffusion, labor, urbanization, and political and then social and cultural factors. There is also some attention to the "costs" of growth in underdeveloped social provisions and pollution, and much discussion of future possibilities.

A fundamental problem encountered in nearly all the chapters (and indeed in most writing on Japan) was neatly stated by Henry and Mable Wallich: "it is tempting to stress differences and to find that Japan is indeed unique," but "tempting as well to show that economics [or whatever] works in Japan as it does elsewhere" (p. 253). One's conclusions on this point will always be determined as much by approach as by the "facts" observed. The editors' standpoint is clear—they emphatically if "gently suggest that Japanese growth was not miraculous: it can be reasonably well understood and explained by ordinary economic causes" (such as the stock of educated labor, largely in low-productivity jobs, at the beginning of the period and a favorable position to import technology—pp. 6, 12). Several authors follow their lead, arguing, for example, that patterns of industrial concentration, of taxation, of urbanization in Japan are not as nearly unique as most people think. The opposite side is taken by a sociologist, Nathan Glazer, whose chapter on social and cultural factors is mainly a thoughtful review of the considerable western literature on Japanese education, value structure and workplace organization. Glazer maintains that "traditional" Japanese elements remain important today, and have had a *positive* effect on growth which may disappear as modernization proceeds. Other authors too emphasize features of the financial system, the handling of foreign trade, saving behavior and so forth which may not be exempt from the laws of economics, but yet appear culturally determined and perhaps uniquely Japanese.

A focus for such concerns is the question of the government's part in economic growth. The chapter devoted to this topic specifically is a strong rejection of the "Japan Incorporated" stereotype that Japan is dominated by a single-minded political-business-bureaucratic "power elite." Philip Tresize and Yukio Suzuki argue that the Japanese government is actually quite pluralistic or even fragmented, that it has pursued many objectives besides (or even contradictory to) rapid economic growth, and that its role may not be very different from that of several European governments. But though all the authors probably would agree with the editors' view that "the main impetus to growth has been private" (p. 47), still, several do stress the active, skillful and sometimes quite distinctively Japanese part played by government in—for example—macroeconomic regulation (Gardner Ackley and Hiromitsu Ishi), encouraging imports of technology (Merton J. Peck and Shūji Tamura), and generally making the best out of adverse situations (Lawrence B. Krause and Sueo Sekiguchi). The variety of specializations and viewpoints included here gives the reader a sense of how complicated the relationships between government and economy in rich countries have to be.

This book is valuable at three levels. First, it provides an amazing amount of information hitherto available only in hard-to-find (or to read) sources. Second, some of the chapters generate new knowledge—in particular, Edwin F. Denison and William K. Chung (on apportioning the sources of growth) and Richard Caves and Masu Uekusa (on industrial concentration and its effects) report important new research. Expanded versions of both chapters, incidentally, have since appeared as separate books. But the most noteworthy contribution of the project lies in between these two levels, in interpretation, assessment, and reformulation of ideas. The authors are so secure in their command of economic theory and the relevant experience of the West that they can move confidently through masses of evidence, asking the *right* questions and fitting the answers together in ways that make sense. Such scholars as Joseph A. Pechman (with Keimei Kaizuka) on taxes, Walter Galenson (with Konosuke Odaka) on labor and Edwin S. Mills (with Katsutoshi Ohta) on urbanization write "authoritatively" in the best sense of the word. The expensive and elitist Brookings strategy is well vindicated.

The book has more than its proper share of small errors—the funniest is having Hideo Sato, then a Brookings political scientist, run for the presidency of the Liberal Democratic Party—and it is too bad the editors lacked energy for a real attempt to contrast and compare the authors' varied findings and opinions. Quibbles aside, I think I have learned more from this volume than from any other single book on Japan, and recommend it enthusiastically to Japan specialists and to anyone seriously interested in economic public policy.

JOHN CREIGHTON CAMPBELL

University of Michigan

Socialist Opposition in Eastern Europe: The Czechoslovak Example. By Jiri Pelikan. Translated by Marian Sling and V. and R. Tosek. (New York: St. Martin's, 1976. Pp. vii + 221. $12.95.)

Jiri Pelikan's book concerns the prospects for a viable socialist opposition in Czechoslovakia with some application to Eastern Europe and the Soviet Union. While he speaks as a participant observer—he was Director of Television in Czechoslovakia from 1963–1968—his analysis of the organizational and ideological development of a socialist opposition has moral *and* analytical appeal. It is a valuable (though not definitive) effort to come to grips with the problems of developing a durable opposition in a communist state.

Pelikan's analysis is divided into two parts: his own interpretation of the Prague Spring and its aftermath through 1974 (pp. 7–113), and the analysis put forth by the socialist opposition about events during the same period (pp. 117–217). In the first section, Pelikan poses four critical questions, to which he later offers some tentative answers. Could the Czechoslovaks have maintained the victories won in 1967–1968, or was the termination of these reforms inevitable once the Soviet troops invaded in August, 1968? Is the opposition movement a meaningful force today, or was it stopped by the Russian invasion and the subsequent political trials? Does the Czechoslovak road to socialism as envisioned by the opposition apply to other communist nations as well? Finally, how can one nurture an opposition movement in a closed political system? In the documentary section (in which Pelikan carefully places each piece in temporal perspective), similar questions reverberate, though ideological appeals tend to dominate the dialogue. Thus, the documents serve as a springboard for Pelikan's more conceptualized analysis of how the opposition has developed and the problems it faces.

The first issue—the freedom of maneuver open to Czechoslovak reformers in August, 1968—is where Pelikan provides his most controversial insights (pp. 27 ff.). Pelikan asserts that if the Czechoslovak party had remained united and played up world public opinion and domestic Soviet opinion against the Soviet invation, then the Russian "diktat" could have been rejected. As evidence he points to Moscow's hesitation to set up a government of quislings, a show of ambivalence that the Czechoslovak leadership seemed to misunderstand. Second, Pelikan suggests that the elites' immediate decision rule to go along with the Soviets was the fatal mistake. As he summarizes, the supporters of the "New Course" "... seemed to believe that time was working for them, whereas exactly the opposite was true" (p. 28).

It is on the second issue—the durability of the socialist opposition—that Pelikan offers some of his most cogent points. As his documentaty appendix seems to demonstrate, the ideals of and support for a "true socialism" in Czechoslovakia may not be dying (pp. 75 ff. and 135–56). Pelikan asserts that Gustav Husak's "normalization" is creating daily converts to the opposition, and the growth of dissent in the rest of Eastern Europe and the Soviet Union provides continuing moral support for the Czechoslovak opposition. Finally, he points to the decay of the Party in Czechoslovakia, noting that purges and recruitment failures have raised the average age of party members to 50 years. Ironically, by expelling 500,000 members, the Party has provided the opposition with a knowledgeable and committed vanguard (what Pelikan calls the "Party of the Expelled"). Thus, the Czechoslovak opposition movement is, in Pelikan's view, increasing, not diminishing.

Pelikan's optimism is carried over into the third issue he addresses—the relevance of the Czechoslovak experience for other communist nations. He quickly rejects the notion of "Kadarization" as a solution to instability in Eastern Europe (pp. 56–57) and asserts that all communist regimes that deny freedom and democracy and instead practice "bureaucratic socialism" or "goulash communism" will eventually face a crisis of authority. This is because throughout Eastern Europe all major groups in society are fully aware of the gap between socialist ideal and reality (p. 93). The irony that the communist parties, themselves, provide society with a standard of performance that the Party cannot meet is not lost, then, on Pelikan (p. 77).

The final and most pervasive theme that runs through this book is the problem of maintaining a viable opposition in a closed political system (pp. 77–100). Pelikan exhorts dissidents to avoid "over-organizing" and to rely instead on polls of dissent which are linked in an ideological, rather than a structural sense (pp. 47–56). In Pelikan's view, success depends on expanding the movement to embrace mass *and* party in Czechoslovakia, dissidents throughout Eastern Europe, and the West European Left. Thus, for Jiri Pelikan, the road to success for a socialist opposition is *not* through an elitist vanguard, but through a mass (and elite) movement along the lines of the Czechoslovak experience in 1968. Only through such a democratic movement, then, can Czechoslovak society "rehabilitate socialism."

VALERIE BUNCE

Northwestern University

Religion and Politics in Sri Lanka. By Urmila Phadnis. (Columbia, Mo.: South Asia Books, 1976. Pp. ix + 376. $16.50.)

Urmila Phadnis has delved deeply into the interaction between professional religious elites and the contemporary political system in Sri Lanka. This important volume is the fruit of many years' study of Lankan affairs, which includes numerous trips to the island from her base in New Delhi, where she is presently Associate Professor of Politics at the Jawaralal Nehru University. It is a vivid case study of how activists within the community of professional *religieux* have brought influence to bear on secular, competitive politics. She also explores the reverse—how this participation in competitive electoral and legislative politics has secularized certain aspects of the *sangha* (or monkhood).

She first describes with unusual precision the evolution of the Buddhist *sangha*—its organizational, structural aspects, its accommodations to the Lankan caste system, its peculiar landlordism and mode of ascriptive succession typical of the longer-established, wealthier upcountry temples in contrast to the more modern regimes in most low-country sects. The close historical links between the *sangha* and the Sinhalese kings before the coming of the Europeans are perplexing to contemporary lay observers and monks and provide sharply differing views on what should be the contemporary arrangement between the secular government and the religious community. She makes clear the dilemma facing activist monks and lay persons, in that each temple community desires autonomy which makes sustained influence in the political system difficult, while *sangha* resistance to lay influence inhibits outside efforts to regularize financial and other administrative aspects of temple affairs.

Phadnis is ingenious and sensible in her use of Almond's concepts of nonassociational interest group and demand aggregation and Dahl's role analysis and the dynamics of polyarchy to help explain how, despite those structural difficulties, elements of the *sangha* have had so much influence on Lankan politics since independence. The monks' intermediary role between rural village voters on the one hand and the national political parties on the other has given them substantial influence within a highly competitive system based on universal franchise and secret ballot. They have profoundly affected language and educational policies; these in turn have altered recruitment to the civil service and the allocation of opportunity between ethnic communities. Associational interest group strategy has also been sufficiently well directed between elections to prevent the enforcement of legislation which would have authorized the secular government to limit the self-governance of the different temples and various sects.

In 1956, Phadnis reminds us, most of the activist monks supported Bandaranaika's SLFP, and were assumed to be indispensable to his victory, thus bringing great influence to the *sangha*. By 1965, however, activist monks were backing all major candidates, thus dividing the *sangha* into competing political factions paralleling the growing polarization within the ever more politically mobilized population at large. In 1956 the monks helped shape the issues; by the 1965 election, it was the competing parties which defined the issues, influence flowing this time from party to bhikku faction, rather than the reverse. By the 1970 election, she reports, economic issues had become far more central, and language and religious concerns—the earlier issues the monks had been able to rally around—were far less manifest. She traces successive efforts to define acceptable relationships between *sangha* and state and the sources of opinion and the strategies used by differing groups in the *sangha* to promote and to espouse these alternatives.

Phadnis' exploration goes well beyond my own study in the mid-fifties and Donald Smith's contributions to his edited volume

South Asian Politics and Religion (1966). She draws on the fullest possible sources, including the work of Dieter-Evers Gambrich, Obeyesekere, the early historians and government documents and parliamentary debates.

This is the most comprehensive and penetrating study of the relationships between professional religious elites and the secular political processes in a modernizing society that I have seen. One would hope that equally careful and balanced case studies would emerge regarding analogous relationships affecting Islam and Christianity in developing Third World states in the near future.

W. HOWARD WRIGGINS

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The Crisis of the Dictatorships: Portugal, Greece, Spain. By Nicos Poulantzas. Translated by David Fernbach. (London: New Left Books and Atlantic Highlands, N.J.: Humanities Press, 1976. Pp. 166. $10.50.)

Within a discipline not remarkable for its radical tradition, Marxist theory is showing some limited signs of becoming respectable. For American political scientists who rejected the gruel of behavioralism, but found little sympathetic indigenous scholarship to sustain them, European Marxists such as Nicos Poulantzas have been of signal importance.

Poulantzas' new book, *The Crisis of the Dictatorships,* is a preliminary, theoretical attempt to assess the undermining of dictatorial politics in Portugal, Spain and Greece. This process, he argues, must be understood by referring to the relationship between international capital and the composition of social classes within each country. On the one hand, a *comprador* bourgeoisie acted as an intermediary for foreign—overwhelmingly American—capital. Here the dictatorships found their most consistent support. On the other hand, a domestic bourgeoisie, a class of home-grown capitalists, while partially dependent on foreign capital, aimed to further its own interests but was prevented from doing so by the *comprador*-foreign capital alliance. In consequence it preferred to be rid of the dictatorships and to cultivate ties with the European Community, an international capitalist community it saw as offering more leeway for its ambitions.

Next, Poulantzas turns to the "popular classes," which played a secondary role in the overthrow of the military regimes in Greece and Portugal. Their allegiances divided between different organizations; they were unable to launch a frontal attack on the dictatorships. Instead, they joined with the domestic bourgeoisie in undermining military rule and establishing quasi-bourgeois /democratic regimes.

He follows the class analysis with an exposition of the state apparatuses of the dictatorships. In general, the capitalist state's function is to ensure the accumulation of capital for the bourgeoisie as a whole, although different fractions of it have different views of how to further their own particular interests within that overarching objective. The military dictatorships came into existence precisely because no one fraction of the "power bloc" could ensure its own hegemony. The military assured the continuation of capitalism when a fragmented bourgeoisie proved incapable of doing so. But the military states were no more able to escape the contradictions of social classes than their predecessors. Further, they lacked parties and bureaucracies that would have enabled them to exert the kind of ideological and repressive hegemony that one finds in the classic fascist state.

The internal vulnerabilities of the regimes contributed to their collapse. For, faced with a determined attack by the domestic bourgeoisie together with its proletarian and liberal intellectual allies, they found themselves unable to respond effectively. The resulting democratization did not constitute a fundamental change in society, but rather perpetuated capitalist social relations in a new, if less oppressive, form.

Poulantzas' great strength lies in his insistence that we cannot deal with individual countries or with geographical regions without situating them in the world capitalist system. The crucial question in understanding the politics of countries that are part of the network of international capitalism is how the social classes in each political system are related to that network.

This view guides his subtle work on the dominant classes. Yet in his analysis of the dominated classes Poulantzas seems to forget his own warnings. He tends to treat the masses in an undifferentiated fashion. However, there are splits in the working class that derive from the divisions both within international capital (e.g., divergent subjective interests of labor employed in foreign versus domestic-owned corporations) and domestic capital itself (monopoly versus non-monopoly employees). Moreover, there are divisions that reflect the schisms of international socialism (China versus Russia, Moscow versus Eurocommunism). Becuase he investigates none of these, his understanding of class struggle, and thus of the vital issues of the accumulation of capital, suffers badly.

Lastly, as a structuralist Poulantzas seeks to avoid the crude notion that a monolithic ruling class consciously dominates the state, while maintaining that the state nonetheless functions to ensure the reproduction of capitalist social relations. (How else to understand the essential continuities in the democratization of the dictatorships?) But his description of the underlying structure—the "parallel state of network" —that resolves this paradox is hardly satisfactory: "A *network,* as it runs through the various branches and apparatuses of the state; *parallel,* as it functions behind the facade of the state apparatuses, which carefully disguise it; *state,* as although often only para-public, it provides a permanent recourse for the bourgeoisie in their struggle to maintain and safeguard their power" (pp. 100–01). As it stands, the "parallel state network" is little more than a necessary hypothesis to make the general structuralist argument plausible. But what, after all, is the content of this spectral being? Poulantzas needs to be a great deal less cryptic before we can judge the validity of his position.

Such difficulties aside, this is a deep and provocative book. It is much more than a blow-by-blow description of the erosion of the military regimes of Portugal, Spain and Greece. Rather, it is part of a growing corpus of Marxist literature (e.g., the works of Wallerstein, Frank and Anderson) that insists that one can understand nonsocialist comparative politics and international relations *only* by reference to capitalism. And this implies a direct confrontation between the radically divergent paradigms of politics presented by Marxism and mainstream political science.

PETER COCKS

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Committees of Inquiry. By Gerald Rhodes. (London: Allen and Unwin, and New York: Crane, Russak, 1975. Pp. 232. $21.50.)

The British have long had a penchant for governing by committees, from the cabinet down to local government. At the administrative level, interdepartmental committees serve as lubicants of coordination; standing advisory committees link pressure groups to ministerial decision making. A somewhat different creature is the committee of inquiry, composed of non-officials appointed by ministers to investigate, report and recommend on a one-shot basis on subjects ranging from constitutional devolution to decimal currency to herbage seed supplies. Royal commissions are the best known of this type, though not invariably the most important.

Assessment of the value of committees of inquiry is no easy task, and judgments have varied considerably. North American scholars have been generally impressed by them, British ones mixed in their reactions. Politicians often complain about their operation (Harold Wilson remarked that they "take minutes and waste years") but when in office continue to appoint new ones. There is no dearth of existing studies, by (inter alia) Clokie and Robinson, Wheare, Cartwright and Richard Chapman and associates. Much of the literature, however, relies mainly on case studies of the larger, more dramatic inquiries; a good deal of it is dated; and it has not been marked by much systematic social science.

Gerald Rhodes' recent book differs from its predecessors in being more analytic, comprehensive and exploratory. Commissioned by the Royal Institute of Public Administration, it draws on the experience of all 170 committees appointed in the ten-year period 1959–68—grand-scale and mundane alike. Rhodes' purpose is twofold. Using his data he has attempted to test old and develop new generalizations about standard questions such as how committees of inquiry can best be classified; what their purposes are; who gets on them and how they operate; what impact (if any) they have on policy; and, broadly, where they fit into the policy process in Britain. (His work lacks a comparative dimension.) His second concern is methodological. How, he asks, in view of the proliferation of government-sponsored inquiries, the diversity of reasons for their appointment, the idiosyncracy of their operations, and the competition of other sources of advice reaching ministers, can he possibly judge their worth or their place in the development of policy?

The scope and particularity of Rhodes' data is rather daunting, and his accomplishment in tracing the origins, course and outcome of the labors of nearly 200 committees is impressive. His evaluations are balanced, commonsensical and suspicious of grand theory. He calls into question several of the more common assertions about the conduct of British inquiries—that they are usually dominated by a small coterie of "the great and the good," that they are often used by governments to cool pressures and postpone action, that they are a more rational means of getting information than are alternative devices. "The value of a committee of inquiry's report as a source of information about the subject is . . . largely incidental to its central purpose. . . . For the most part, committees have in fact a relatively limited function, which is as much concerned with achieving a workable practical accommodation between interests and viewpoints as with the more fundamental questions about the nature and effects of particular policies" (pp. 207–08).

Observations of this kind are about as far as Rhodes is prepared to go toward generalizing about his maze of committees. Critical of any single method of reaching secure generalizations, he drops hints as to how more authoritative knowledge must be generated, although he fails to pull them together into concentrated advice. Would typologies help? Rhodes coins a few tentative ones, but is aware that they cannot substitute for explanation. Would more case studies help? Again, the answer is yes, to an extent, but simple cumulation of new cases may founder on the issues of representativeness and comparability. How about focusing on the impact of committees on the long-term formulation of policies rather than on the proximate results of single inquiries? He finds this a good prospect, but acknowledges that it is far from easy to isolate the effects of committees' activities from the array of influences on government from all quarters.

Rhodes is well aware of such difficulties in conducting research into this fairly large universe of political activity, but insists that there are more dangers in studying the results of smaller-scale efforts, especially only inquiries that attracted a good deal of publicity. His book, informed, cautious, somewhat dull, is a useful prolegomenon to future research on a subject of perennial interest to students of policy making in committee-prone systems.

JAMES B. CHRISTOPH

Indiana University

The Donegal Mafia: An Irish Political Machine. By Paul Martin Sacks. (New Haven and London: Yale University Press, 1976. Pp. xi + 241. $15.00.)

With the publication of *The Donegal Mafia*, Paul M. Sacks has added a needed contribution to the literature about the Republic of Ireland. Since the independence and creation of the Irish Free State in 1921, this 26-county section of Ireland has provided a fascinating location for the study of parties, constitutions and post-colonial emergence politics. In addition there is the ever-complicating factor of "Irlanda Irridenta," the gnawing concern about Northern Ireland and the border.

Sacks confines most of his study to the politics of the Dail constituency of North East Donegal and to the machine built in that area by the Blaney family. The machine has been given the title, "The Donegal Mafia." Sacks' information is based on almost two years' residence in Ireland and some very intensive interviewing. The uncovering of personal relationships, family and friendship ties, favors given or assumed to have been given in the form of county jobs, etc., makes the book interesting reading and worthy of the attention of sociologists as well as political scientists.

Sacks concludes that the "Donegal Mafia" machine conformed in its main outlines to the usual machine sociology. However, in one area it contrasted sharply with the classic machine. In the usual instance the machine was built on voters from the ethnic fringe; in the Irish case, only in their later mature years did the Irish machine politicians feel secure enough to reach out and mobilize the Protestant ethnic fringe in Donegal. Sacks appears surprised to have found such a machine in Ireland due to the inhospitable political landscape. He points out that "it is quite unusual to find a machine where there are extensive state-sponsored welfare programs, limited stores of political patronage, and a proportional representation (PR) system" (p. 210).

He maintains, however, "that, even in a highly bureaucratized system, a machine may persist if set within the context of a supportive political culture" (p. 224). The culture in North East Donegal was supportive and the collapse of the Donegal Mafia came about only as a result of a great political miscalculation at the national level by the machine leader Neil Blaney.

The conditions found in North East Donegal are still to be found in other parts of Ireland and, the bureaucratic state system notwithstanding, it is not unlikely that a similar machine could evolve in other constituencies.

The material in this book is detailed and focused upon a very local situation and it does not lend itself to undergraduate instruction. Nevertheless, Sacks joins the ranks of Chubb, O'Brien, Rose, Cogan, et al., in bringing a scholarly style to the study of Irish politics. *The Donegal Mafia* is a useful and admirable addition to the literature. For too long the field has been dominated by nationalists waving "bloody shirts" who contributed more heat than light.

HENRY KENNEDY

Florida Technological University

Czechoslovakia's Interrupted Revolution. By H. Gordon Skilling. (Princeton, N.J.: Princeton University Press, 1976. Pp. xvi + 925. $45.00, cloth; $15.00, paper.)

H. Gordon Skilling has added a major work to the 600 books (by his own count) which deal with the "Prague Spring." The scope of Skilling's 925-page book makes it the most ambitious undertaking among the literature dealing with this topic, and has to be viewed as an effort to write the definitive work. It faces competition from such brilliant commentators as Vladimir Kusín and Galia Golan, who address themselves to the same historical phenomenon.

The work is subdivided into six parts which cover every facet of the reform movement. Parts 1 and 2 explain historically the downfall of the Novotný regime in January of 1968. Part 3 is devoted to a chronology of events in the evolution of a new system. Parts 4–6 depart from the historical approach and provide a topical treatment of the major issues which animated the Czechoslovak polity during those turbulent days. Part 4 deals with the systemic changes which were designed to reconstitute one of the most monolithic systems in East Europe in a more pluralistic mold. The most important of these issues was the reconstitution of the Czechoslovak judicial system in the wake of the revelations about the purge trials of the eary 1950s. These events hung like Damocles' sword over the entire Novotný era. The Czechoslovak Communist party had to face the issues arising from the trials before it could restore legitimacy to the administration of justice. The author discusses the Slovak problem and the issue of federalism as Slovaks confronted the reform leadership. He points out the differences between Czech and Slovak political leaders. Czech Communists viewed the reform movement as a vehicle for the achievement of a socialist democracy, while many Slovaks were primarily concerned with making the two nationalities equal. The author also analyzes the reforms designed to put the entire economy on a more viable and market-oriented basis.

Part 5 deals with the various publics, party and non-party, which supported or opposed the reform movement. The author draws very heavily on the results of polls which were widely conducted during the reform era. Part 6 places Czechoslovakia and the reform movement in the context of international cross-pressures which finally terminated the movement with Soviet occupation. It draws a careful account of the positions of various Communist parties within and without the Soviet bloc, and the positions of individuals who influenced the decision to intervene.

The work is a monument to careful scholarly research. Skilling refrains from a very active analysis of his materials, introducing such a profusion of events, documents and personalities that the reader has to depend on prior knowledge to come to cogent judgments. The political leaders appear on the scene only as segmented personalities without development of their previous records in depth. The short period of the Prague Spring commands so much space that the historical introductions are not equal to the task of preparing the reader for this culmination of 20 years of political evolution.

Like practically all authors dealing with Czechoslovakia, Skilling pays homage to the Czechoslovak democratic tradition (p. 11), but his work casts doubt on the validity of this tradition. Czechoslovak democratic parliamentary experience is confined to two relatively brief interregnums: 1918–38 and 1945–48. Czechoslovakia, with its high degree of industrialization, had more of a bourgeois political base than the surrounding East European states, yet the few years of democracy contrast with the centuries of dominance by absolutist systems imposed by foreign masters on both the Czech and Slovak peoples. The Communist party is an embodiment of both radical and egalitarian strains which are as much a part of the Czechoslovak political tradition as democracy. The very size of the Czechoslovak Communist party presented it with an unchallenged monopoly of power which rendered the Czechoslovak model more intolerant than those found in most surrounding Communist states which had to contend with a wider range of veto groups. It was the crisis of the Communist system rather than the Czechoslovak democratic traditions which caused the reform movement.

The author essentially accepts this position, yet he proposes the thesis that the Soviet occupation aborted a movement which was beginning to assume the dimensions of a revolution, given the parameters of his cautious definition. He does not see it as a revolt of apparatchiks who were trying to stave off the deluge by revitalizing the party. Many of the stars of the Prague Spring had been faithful executors of the policies of the previous era insofar as they had not been rehabilitated victims. They indeed achieved a revitalization of the party in the absence of an organized opposition from nonparty and antiparty sources. These latter elements were willing to go along only as long as the reformers were moving in their direction. The true test of democracy for the reformers would have arrived if they had had to face an opposition presenting a systemic challenge. The previous examples of Poland and Yugoslavia with the process of democratization did not hold out much hope in that regard. The chances are excellent that the reformers would have interrupted the march of change even without a Soviet intervention, had their own positions been challenged. Many of the reformers have since repudiated their erstwhile cause and have participated with varying degrees of enthusiasm in the post-occupation regime. President Gustáv

Husák is an outstanding example.

Skilling sees the Prague Spring as a possible prelude to the continuation of the reform processes and almost as a moral victory. Such an idea has a hollow ring for the tens of thousands who have been deprived of their usual means of livelihood because of their political positions. It is merely the latest chapter in the continuing Czechoslovak tragedy. However, despite these polemical exceptions, Skilling's book is a masterful contribution which will stand out in a vast field of literature.

GEORGE KLEIN

Western Michigan University

Forging Nations: A Comparative View of Rural Ferment and Revolt. Edited by Joseph Spielberg and Scott Whiteford. (East Lansing: Michigan State University Press, 1976. Pp. vii + 168. $8.50.)

Joseph Spielberg and Scott Whiteford's *Forging Nations* is a collection of several illuminating essays on comparative peasant movements. The essays focus on the ways peasants have experienced the spread of capitalism and the growth of central states and the seemingly hopeless struggles of rural people to hold on to their traditional ways of life or to gain greater power over their destinies *within* these ongoing processes.

Charles Tilly's essay on "Rural Collective Action in Modern Europe" argues that peasants in Europe not only paid for the growth of capitalism but also lost their peasant way of life in doing so. This in spite of their sporadic collective actions against the market and the state. Modern state making overwhelmed the autonomy of peasant communities, as it has done in most of our world, and the repressive power of European politics defeated the peasants in their collective actions to restore the world they were losing. Force plus proletarization in urban industrial economies did them in, and the tragedy of Europe's peasants is that their victorious "proactive" protests for a share of national power within modern capitalist states produced a proletariat that is ultimately subordinate within the modern capitalist world.

J. H. Broomfield deals with a peasant revolt for modest subsistence goals and shows why and how radical Marxist intellectuals lost who tried to command this movement in Bengal. Not only were powerless local people skeptical about outside agitators without an armed force that could protect them from police reprisals, but also that force, when linked to patronage and central power, was able to divide and rule. Patronage and guns will continue to make rural Bengal a tragedy for the poorest of the poor.

Fred Katz's contribution on the Mexican revolution argues that the limits of Zapata's and Villa's regional power centers in the face of a strong (enough) centralized state that could crush, and with the help of the U.S., purchase the support of less militant rebel forces. The national government countered the power of regional caudillos by incorporating their peasant movements into popular (read government-penetrated) unions and sending the army to constrain their more militant activities.

The truly outstanding contribution to this volume is John S. Saul's essay on "African Peasantries and Revolutionary Change." Taking issue with Marx, though not realizing that Marx was almost entirely wrong on the revolutionary potential of rural people, Saul recognizes the importance of traditional peasant self-reliance rather than dependence in the making of family subsistence, and suggests that peasants are hesitant to participate in capitalist economies where that entails giving up the family's means to subsistence in the old village world. Saul asserts that peasants are inclined to avoid centralizing governments, cash-cropping for world capitalist markets (which is usually coerced), and nationalist or socialist movements devoid of land and bread. Though Saul would like to challenge the notion that spontaneous peasants are imperfectly capable of self-governing endeavors without the intervention of superior commandist bureaucrats, Saul's account of Mozambique and Tanzania somehow causes him to hedge here and conclude that they still need political guidance.

John Shockley's fine essay on "The Crystal City Political Revolts" focuses on the limits of change that migrant workers could achieve even with the assistance of unions in South Texas. Mexican officials let strike breakers cross the border and the Texas Rangers jailed the migrant worker leadership. This, plus their low levels of political education about how the electoral institutions of the dominant culture work, undercut their attempts to take and hold power.

These perceptive pieces beg a number of theoretical observations, but here I will offer only two thoughts on what seem to be the recurring themes in the whole effort: the overwhelming odds against peasant revolution in the modern world and the trend toward a non-peasant, predominantly capitalist, world.

Though these scholars point out that peasants fight back against the modern world system, they almost uniformly assume that peasants are bound to lose. That may be true. Neither peasants nor proletarians have made earth-shaking revolutions *in* the modern world.

But there are revolutions and rural people have provided the "steam" for them, to use Saul's term. Who, then, led these movements? The answer is the uprooted people who can no longer scratch it out in the old village world or find a subsistence niche in the new factory world. The vanguard of China's modern rural revolution, to take the most important earth-shaking experience of our time, were just such uprooted paupers in limbo. Though western social scientists have seldom studied the growth of revolutionary political movements among these uprooted dangerous elements in peasant countries, Tilly's essay suggests that it is the uprooted who are the most militant victims of the modern world system: "In Europe peasants who found themselves on the path to rural wage labor, but still had some claims on the land, seem to have had a special propensity to struggle" (p. 34). Richard Cobb has touched upon this propensity in France 1789–1815 in his brilliant study of *The Police and the People*. Similarly, Saul claims it was the "semiproletarianized hangers-on" who comprised part of the vanguard in the Guinean revolution (p. 101). It is, then, the least integrated elements, the ex-peasants but not yet proletarians who are no longer a part of the pre-capitalist village and who remain outside of the capitalist market system, who can forge a militant resistance against modern state making that *can* win if the objective conditions are right—such as splits in the national elite and imperialist powers divided by major wars.

This brings us to a closing point. There is no a priori reason why we must accept Eric Wolf's assumption, as does Tilly, that peasants will be destroyed by the revolution they help bring to power. There is no denying that such was the case in the Russian Revolution. The Bolsheviks did not respect the demands of the peasants for autonomy from the rapidly developing party center. Trotsky's Red Army crushed the rural poor through its very objective murderous methods of mobilization and extraction. Though recognizing such peasant desires for autonomy, these essays do not, by and large, consider that there is another tradition that carries the promise of peasant revolution, one that runs counter to capitalist and communist systems of state making—anarchism. And what if the uprooted who join together in arms do not integrate the peasants into a new extractive state but rather build a social revolution through protecting and enhancing the Little Tradition Archisms which give power to local peoples? That is the continuing relationship of state and revolution in modern China. That the uprooted mobile armed center can win without warring on the peasants is the essence of the first rural revolution to occur on the periphery of the modern world system, where capitalism and colonialism were weak and divided. And do not be surprised if the moral and political message of that uniquely Chinese experience resonates with, increasingly in world-wide conscious ways, the dreams and struggles of the victims of a world that may indeed, as Tilly claims, be predominantly capitalist but is turning backward toward the promise of powerless peasants gaining power and remaking their world in the image of their richly varied peasantness.

RALPH THAXTON

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Yearbook on International Communist Affairs: 1976. Edited by Richard F. Staar. (Stanford, Calif.: Hoover Institution, 1976. Pp. xxx + 636. $25.00.)

Works such as this should be reviewed as they are written—by a team of specialists individually competent to examine the country-by-country descriptions of Communist party activities for 1975, the year under review in this tenth volume of the series. Barring that development, a single reviewer at least ought to confess his limitations. My talents, such as they may be, are limited to the Soviet Union and Eastern Europe, and in diminished measure to China and the major parties of Western Europe.

For readers not already familiar with this work, the *Yearbook* reviews the major developments within and among world Communist parties and front organizations. Individual national profiles deal with party status (governing or nongoverning, legal or proscribed, etc.), membership and electoral strength, leadership, auxiliary organizations, domestic activities, economic issues, ideology, international relations, and orientation in the Sino-Soviet dispute. When applicable, discussions of pro-Russian, pro-Chinese, or other major factional divisions are provided. The end result is a comprehensive and exceptionally valuable reference work for any student of Communist affairs.

Coverage of Eastern Europe and the Soviet Union is fairly uniform, although the essays on the German Democratic Republic and Poland stand out in terms of presenting a balanced and closely knit overview of economic and political developments. Unfortunately, the section on the USSR, which should emerge as the centerpiece, lacks that careful balance; attempts at economic and internal party rationalization are interpreted essentially as control issues rather than as a part of the larger question of modernization. Too much attention is given to the dissident community, and far too little to

Sino-Soviet relations. For the region as a whole, however, some common themes emerge: (1) the salience of modernization-related issues and difficulties in defining a model of a mature socialist state; (2) continued problems with the economy, centering on flagging growth rates, political and economic problems with managerial reforms (reversals in Hungary's NEM, attempts to develop viable regional associations in the USSR and Poland, and a clash between the party and managers in Albania are examples), and resource and fuel problems, especially in Eastern Europe; (3) firmer control over intellectual life and dissident elements, although with varying intensity; and (4) the preeminence of the USSR within the bloc, expressed through pressure for economic integration through COMECON (with Rumania the predictable exception) and the dominance of the Soviet position on the major issues raised at Helsinki, although Soviet concessions on the terms for the meeting of European parties must be weighed on the other side of the balance.

The section on China captures well the tone of an unstable conservative restoration after the Cultural Revolution and does a much better job with the Sino-Soviet dispute. The chapters on the major European parties reflect the ambiguities which face Communist parties within parliamentary systems and in dealing with potential socialist allies.

Two additions would, however, increase the volume's usefulness: (1) a more extensive overview essay than is presently offered, and (2) separate sections focusing on COMECON, the Warsaw Pact, and major international party conferences.

DONALD R. KELLEY

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Prelude to Self-Government: Electoral Politics in Papua New Guinea 1972. Edited by David Stone. (Canberra: Australian National University and University of Papua New Guinea, 1976. Pp. x + 547. $AU 8.00, paper.)

Although Papua New Guinea gained independence only in 1975 (Papua had been an Australian colony, New Guinea an Australian Trust Territory), it has already had more experience with national elections than many of the "older" new states. The first was conducted in 1964 after some prodding of the Australians by the Trusteeship Council, to be followed by others in 1968, 1972, and 1977. By international standards, there was very little at stake in the first two elections, for independence was not yet on the horizon, and power was to remain in Australian hands. By 1972, however, there was every indication that self-government—an intermediate stage between colonial rule and full independence—was imminent and independence inevitable, probably during the life of the Third Parliament. This is not to imply that by 1972 Papua New Guineans were united in any conventional anti-colonial, nationalistic sense. On the contrary, it was more a situation of the Australians pushing self-government and independence upon a tribally divided, linguistically diverse, and politically unmobilized people who, according to all assessments, for the most part did not want the Australians to set them free, and were fearful of what indigenous rule might bring.

While these elections have passed virtually unnoticed by the rest of the world, they have received considerable scholarly attention, particularly by Australian social scientists. Each of the first three elections has been followed by a major book published by the Australian National University Press—the Stone book under review here; A. L. Epstein, R. S. Parker and Marie Reay (eds.), *The Politics of Dependence: Papua New Guinea 1968* (1971); and David G. Bettison, Colin A. Hughes and Paul van der Veur (eds.), *The Papua-New Guinea Elections 1964* (1965).

In quite a number of respects, Papua New Guinea elections have been studied differently from elections in most other Third World or developed countries. Briefly, these differences include (a) a focus on individual constituencies as the unit of analysis, (b) a focus on candidates rather than parties, politically relevant organizations, issues or ideologies, (c) extensive involvement in the field research by social anthropologists or others familiar with the socioeconomic and historical background of the electorates, (d) the participant-observer approach, (e) virtually no systematic public opinion polling, (f) little concern with systematic analysis of the voting data, and (g) virtually no concern with research designs, common conceptual frameworks, or with systematic, cross-constituency hypothesis testing and comparison.

The Stone book contains case studies of widely varying length and quality of 34 of the 100 constituencies, authored by some three dozen writers, including, for the first time, indigenous scholars. For the most part, the case studies are purely descriptive accounts of the constituency, the candidates, the nature of the campaigning, and the outcome. But the absence of a set of common research foci, and the atheoretical, conceptually barren case study approach prevents the reader (and evidently the editor also) from offering system-level generalizations and explanations. Explanations of the victory or defeat of candidates are often couched in such idiosyncratic terms as to prevent even cross-constituency, intra-nation comparison, much less comparison with elections elsewhere.

Another major shortcoming of the Stone book, and of its predecessors, is the failure to explain even in a minimally satisfactory manner why Papua New Guineans voted as they did. The only effective way to answer this question is to ask a cross section of citizens why they voted as they did. Yet no common survey instrument was developed, and only six of the authors made use of any kind of questionnaire. Nonetheless, this did not deter the majority of the authors from subjective assessments of public opinion. Yet such armchair rumination, even if conducted in a village in the field, is simply no substitute for public opinion polling.

The studies of the 1964 and 1968 elections have not found a place in many collections of research on Third World elections. The Stone study, even more deeply embedded in the case study mold, is likely to suffer a similar fate.

GEORGE H. GADBOIS, JR.

University of Kentucky

Political Leadership in Korea. Edited by Dae-Sook Suh and Chae-Jin Lee. (Seattle and London: University of Washington Press, 1976. Pp. xvi + 272. $9.50.)

This book is of special relevance to students of comparative political development and to area specialists on Asia and Korea. It is an outgrowth of two symposia held in Seoul, 1971 and 1972, assisted financially by numerous organizations including the Ford Foundation, the Institute for Comparative and Foreign Area Studies of the University of Washington, the Joint Committee on Korean Studies of the SSRC-ACLS, the Asiatic Research Center of Korea University and the Center for Korean Studies of the University of Hawaii. Although of mixed quality in terms of promise and achievement, the book is definitely a significant addition to the growing literature on Korea that utilizes social scientific methodology.

Following an editor's caveat, that the volume "is not a biographic sketch of important leaders . . . nor is it a systematic exposition of all major phases of political leadership" (p. xi), the book deals with the various types of political leadership in Korea. These include discussions on traditional polity (James Palais), legislative system (Chong Lim Kim and Byong-kyu Woo), party system (Bae-ho Hahn and Ha-ryong Kim), bureaucratic elites (Dong-suh Bark and Chae-jin Lee), mass public (Sung-chick Hong and Young Ho Lee) in South Korea, and Communist party (Dae-sook Suh) and top-power echelon (Chong-sik Lee) in North Korea. There is a concluding chapter (Glenn Paige) entitled "Toward a Theory of Korean Political Leadership Behavior" and an appendix by the same author entitled "Toward a Political Leadership Profile for a Changing Society." Either one of the last two, with some modification, could have served as a theoretical overview and framework for the volume. In the absence of such an endeavor, the volume generally suffers from lack of cohesion and uniformity of themes. Nevertheless, each chapter is interesting and provocative.

The Yi Dynasty Korea (1392–1910) generally suggered from the abence of charismatic leadership which was attributed by Palais to the structure of the Yi Korea's traditional polity, a centralized bureaucratic monarchy, and the interplay of various political forces in society. It is refreshing to hear a bold argument that "one of the major features of the Yi dynasty polity was the relative weakness of the monarchy and centralized authority because of the aristocratic, bureaucratic and normative restrains on royal power" (p. 3). This is a rather unconventional view, contrary to the prevailing hypotheses of centralization and homogeneity, of the Korean political culture.

The next four articles are based on the findings of survey research of both elite and mass samples in South Korea. In the study of legislative leadership, Kim-Woo discovered "the inverse correlation between the level of formal education and the degree of democratic commitment" among the lawmakers of the Seventh National Assembly (p. 64). They also assert, based on information about legislative output between 1948 and 1967, that legislative autonomy vis-à-vis the administration did not decline but rather "fluctuated widely over the years" (p. 63), the trend which may no longer be the case since the 1972 reform resulting in the strengthening of the power of the presidency. Party cadres in Korea, according to Hahn-Kim, generally exhibit mixed motives for joining the party that reflect considerations more for material reward, job seeking, power and status enjoyment but less for political ideology. In view of the low level of party organization and coherence, and a high degree of discrepancy between rhetoric and reality of party politics as perceived by party cadres, the authors assert that "Korean political parties are likely to remain ephemeral organizations with very limited functions to perform within the political system" (p. 85). The primary blame for this result is attributed to "the reliance of the top-level leadership in power on nonparty organizations as expedient control and suppression" (p. 85), a conclusion which is widely accepted as the cause for nondevelopment of democratic parties in South Korea.

Bureaucratic elites exhibit the "general propensity to place a lower priority on democratic political norms than on national security and economic growth" (p. 130), according to Bark and Lee. While 43.4 percent of their sample of higher civil servants served in the military, 66 percent of this number were those who entered the bureaucracy by special appointment rather than by competitive examination. Interestingly, they discovered little differences in development orientation "between the ex-military bureaucrats and professional bureaucrats" but not necessarily so "between ex-military bureaucrats who had retired before and after the military coup" of 1961 (p. 130). In their study of popular perceptions of political leadership, Hong and Lee discovered "a strong undercurrent of political alienation in Korean society" where leaders are generally "perceived to be unresponsive and illegitimate, and government officials to be self-seeking, unfair, not law abiding and inefficient" (p. 152). Such negative feelings are more pronounced, they claim, among urban and educated segments of the population like university students and journalists, and less so among legislative aides and high school students. Political alienation in Korea, in their view, "will probably diminish through actual improvement in performance" by the government more than "from lowered expectations" of the public toward the government.

Particularly noteworthy in this volume are two pioneering studies on North Korean politics. In the study of party elites, Suh presents hitherto unavailable data on the membership of the central committee of the Korean Workers' Party. An analysis of the pattern of change in the Central Committee composition, based on the groupings of various factions, gives Suh several substantive conclusions. He argues, for instance, that "a large number of newly recruited young leaders ... trained by Kim Il-song and his partisans" replaces the "old revolutionaries who were trained abroad," and that there is in North Korea the "disappearance of factional groupings" (pp. 186–87), a logical and inevitable consequence following the trend described earlier. The recent change in North Korean leadership structure, since the adoption of the 1972 constitution, is carefully examined by Lee in terms of the ranking, age, functional specialty and membership in various decision-making organs of the party and state. The pattern of elite mobility suggests, according to Lee, "a trend toward functional specialization among the top elite." The 1972 constitution, which elevated Kim Il-song as president of the republic in addition to his unassailable role as the party leader, is nothing more than the overhauling of "the political structure of the DPRK in order that the structure of authority would conform to the reality of power distribution" (p. 194). When a corresponding move of power consolidation by President Park in South Korea is taken into account, one wonders if the system-convergence of the two Koreas' political systems in 1972 took place by design or by default.

Political leadership is conceived by most contributors to this volume as properties of a position occupied by various politically relevant strata. An alternative approach may be to treat political leadership as a behavioral pattern of reciprocal interaction between the two role sets of leadership and followership. This volume is not a comparative analysis of political leadership and development of the two Koreas which aims at linking specific findings to theoretical literature on leadership studies. Such a task still remains to be undertaken in the days ahead. In the meantime the Suh-Lee volume provides valuable data to help us move to such a direction of comparative political development studies.

YOUNG WHAN KIHL

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Dispersing Population: What America Can Learn from Europe. By James L. Sundquist. (Washington, D.C.: Brookings, 1975. Pp. xiv + 290. $9.95, cloth; $3.95, paper.)

In *Dispersing Population, What America Can Learn from Europe*, James L. Sundquist examines policies considered or implemented by several European countries to disperse their populations from highly congested urban centers to less populated areas. He then notes lessons from their experience for the United States, should it undertake a similar policy. The value of this book is to introduce the reader to the social, economic, and political complexity of any population distribution policy.

Sundquist notes four basic policy approaches used with varying degrees of emphasis in European countries to reverse or at least stem migration flows to certain urban areas: (1) infrastructure measures to prepare areas of out-migration for industrial growth; (2) incentive measures to induce private investors to go to those areas; (3) controls and persuasion to discourage investment in the major metropolitan areas; (4) direct governmental action to locate its own activities in desired growth areas. The main body of the book develops in detail how these policies have been implemented in several European countries. The discussion is informative and well researched, although better organization would contribute to easier reading and comprehension.

Two questions strike me after reading this book: (1) Is it desirable for the United States to embark on a similar population redistribution policy or policies? (2) How comparable or relevant are the European and American experiences? With regard to the first question, important points of consideration include whether population redistribution is a desirable objective from economic, social and political points of view and whether there is public demand for population distribution. Sundquist concludes affirmatively on the former; about the latter, he finds that it is presently not a very salient issue nor is public opinion on this matter very clear-cut. Two specific factors bring into question whether such a policy is even needed in the U.S., at least at the present: (1) the decrease in the birth rate in the U.S. and (2) the spontaneous redistribution of people in the U.S. away from the older population centers over the 1960–1970 period (p. 248). In sum, the book does not persuade this reader of the pressing need for concerted national public intervention in this area, at least at this time.

The first problem aside, the second question also involves several considerations. One of these is the degree of comparability between the American and European problems of demographic distribution. In most of the European countries, a single metropolitan center dominates a country, with singular flows in migration. In contrast, the migration flow in the U.S. is multi-directional, which complicates the problem considerably (p. 242). A second factor is the difference between the U.S. and any of the European countries in size, both in terms of geographical distances and population levels, not to mention the greater cultural homogeneity in some of the European countries examined by Sundquist. A third consideration is the difference in political traditions which predispose Europeans to accept national planning and public intervention in the private sector to a much greater extent than do Americans. A fourth concern is whether or not the Europeans have really been successful. The answer to that is mixed, depending on the country and the particular policy goal in question. For the most part, however, Sundquist does not evaluate the various policies because of a lack of systematic government information. Yet, it is this type of knowledge which would be most helpful to American policy makers, should they embark on such a policy—keeping in mind the constraints in comparing the U.S. and Europe. More systematic evaluation research on the costs and benefits of the four different policies used in Europe would be of particular interest.

A few general observations suggested by Sundquist are of interest to American policy makers. The experience of Sweden and Netherlands, in particular, points to two stages in European population distribution policy. In the first stage, the object of policy is simply to curb the flow of rural or small-city migration to major metropolitan areas. The second goal (after the first is reached) is to reduce the rate of metropolitan growth to zero and even below zero. Considerable political support exists for the more easily accomplished first goal; much less for the latter (p. 235). Large metropolitan centers may turn out to be just as sensitive to the consequences of population decline as poor rural areas, although this sensitivity probably depends in part on the types of population which leave the metropolis. If urban policy makers find that the decline in the population is primarily among sectors of the population which do not pay much in taxes whild consuming high levels of city services, their loss is not mourned. On the other hand, a decline in population for which the consumption-revenue ratio is the reverse is obviously not desired.

Another general observation made by Sundquist also has implications for U.S. policy makers. Early efforts to disperse population in Europe usually focused on narrow objectives such as relieving localized unemployment rates. More recently, goals have broadened to include reduction of interregional population flows and in some cases planning the geographical distribution of jobs and population (p. 241). Among a variety of policy measures, the latter requires comprehensive regional development planning. This has been less successfully achieved in most of the European countries. New regional political units usually do not have enough political clout to counter effectively claims of both local and national political arenas. While the U.S. is a federal state, the potential problems are the same. Sundquist argues that while states may implement policies for intrastate population redistribution, this problem requires multi-state planning undertaken by regional bodies. How such bodies would be funded and constituted and how much political power they would have would determine to a great extent how effective any population distribution policy would be in the U.S. Since it is likely that these bodies would have little power, the prospects for such policy are not good in the near future.

KATHLEEN PEROFF

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Between Center and Periphery: Grassroots Politicians in Italy and France. By Sidney Tarrow. (New Haven: Yale University Press, 1977. Pp. xv + 272. $15.00.)

Sidney Tarrow's new book on politics in the provinces of France and Italy is perhaps the most impressive contribution yet to the burgeoning literature on comparative local politics. Moreover, it calls into question some important elements in our common understanding of politics in contemporary European states. Well written, theoretically informed, sensitively comparative, it should be on this year's short list of required reading for anyone interested in local government, Western Europe, or theories of political development.

Village life in southern Europe has been the subject of a rich tradition of anthropological case studies, exemplified by Carlo Levi's account of his exile in Lucania and Lawrence Wylie's classic description of a Vaucluse community. Tarrow's work departs from this literature in three significant respects: (1) it is systematically comparative, drawing evidence from matched samples of 117 French and 131 Italian communities of between 500 and 50,000 inhabitants; (2) it uses the methodology of contemporary political science, including standardized interviews with mayors in each community, as well as a wide array of social, economic, and budgetary data; and (3) most important, its theoretical focus is not limited to informal, internal community politics. Rather, the key question for Tarrow is this: What are the effective institutional links between these peripheral communities and national decision makers at the center?

This theoretical point of departure leads directly to one of the study's most interesting conclusions. Grassroots political leaders in France and Italy are best seen, according to Tarrow, not as the traditional notables described by anthropologists, nor as the antiseptic administrators of the jurisprudential literature, nor even as the partisan zealots familiar in political science writings on these countries, but rather as "policy brokers," actively seeking outside resources (especially from the central government) that will enable them to respond to local demands for community development. Students of American local government familiar, for example, with Dahl's portrait of Mayor Lee of New Haven would find little that is surprising about Tarrow's protagonists. Yet it is striking indeed to discover such figures amid the highly centralized bureaucracies, the ideologically fragmented parties, and the traditional social structures of France and Italy.

What strategies are used by these "policy brokers" for achieving their goals? Here a remarkable cross-national contrast appears. French mayors are typically "administrative activists," operating within a well-defined and efficient national administrative system, shunning partisanship, and enjoying close, productive relations of virtual complicity with the prefecture. This strategy implies a relatively elitist pattern of authority, both in the sense that the aura of apolitical administration tends to suppress local political debate, and in the sense that discretion over policy priorities is left in the hands of national technocrats. Tarrow shows that under the Gaullist regime this discretion was used to concentrate resources on the most rapidly developing communities at the expense of declining areas.

By contrast with this *dirigiste* pattern of center-periphery linkage, Italian mayors operate within a more diffuse and politicized system of *clientelist* linkages. In the absence of coherent national policy and of efficient national administration, Italian local officials are forced to become "political entrepreneurs," seeking resources through a network of party contacts. Interestingly, Tarrow argues that contrary to a common image of Italian politics as cleaved into mutually isolated subcultures, these networks of political exchange often cross party lines. One result of this type of center-periphery relations is that national resources are more diffusely distributed than in France, with perhaps a slight bias toward smaller, declining communities. Another concomitant is a more open, politicized pattern of local governance. Tarrow concludes that "the Italian system is more competitive and representative of partisan tendencies than the French one but it is less rational in its use of resources and does little to enhance the reputation of a state whose legitimacy hangs by a thin thread" (p. 248).

Why this sharp contrast in modes of center-periphery linkage in two political systems generally thought to be quite similar? Tarrow argues persuasively and in some detail that the primary explanation is to be sought in the countries' contrasting paths of political development, paths that have produced a powerful state and weak parties in France, a weak state and powerful parties in Italy. If correct, this argument has important implications for the well-known debate among students of political development about trade-offs between strongly institutionalized bureaucracies and strongly institutionalized parties.

Good books raise questions, and this one is no exception. Tarrow's tale is told almost exclusively from the perspective of local mayors, a sensible restriction for a single study. However, one now wonders about the role of other participants in the center-periphery game—party leaders, national deputies, local and central bureaucrats, leaders of agricultural, industrial, and labor organizations. But perhaps the most important unanswered question involves one very puzzling finding of this study: Despite decades of control of these national governments by strongly anti-Communist forces, in neither country have state loans and grants been distributed disproportionately to communities controlled by supporters of the national government. In fact, in Italy Communist communes apparently have received significantly *more* state aid per capita than communes controlled by the Socialists or Christian Democrats!

Tarrow's commentary on this astounding discovery is brief and unsatisfying, particularly since the discovery seems to contradict his interpretation of party as the key linkage between Rome and the periphery. The finding, he comments tersely, "tells us two things: that [the Communists'] political energy and skill are substantial and that, under the Christian Democrats, political control of the bureaucracy was so poor that even the opposition could gain hefty state contributions to the municipalities it governed" (p. 106).

The wizardry of Italian Communist politicians has of late become a commonplace and thus might seem to need no further explication or substantiation. But that the Christian Democrats have failed to use Italy's bureaucracy for partisan ends is hardly a commonplace—*al contrario*! Surely this serendipitous discovery of a seemingly pro-Communist bias in Italian subventions to local government cries out for replication and explanation. If indeed the Christian Democrats have not succeeded in systematically channeling public funds toward their grassroots supporters, we will need to revise radically the standard explanation for their longevity in office, a longevity almost unparalleled among genuinely competitive political systems.

Thus, in a number of important respects Tarrow's book should stimulate a reexamination of much received wisdom about French and Italian politics, not merely at the local level. Furthermore, taken in conjunction with the extensive literature on intergovernmental relations in the United States, as well as such foreign examples as Jerry Hough's classic description of the "Soviet prefects," Tarrow's work may provide the basis for a comprehensive theory of center-periphery relations in advanced industrial nations. In short, this is an unusually valuable book.

ROBERT D. PUTNAM

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Civic Education in Ten Countries: An Empirical Study. By Judith V. Torney, A. N. Oppenheim, and Russell F. Farnen. (New York: Halsted, Wiley, 1975. Pp. 341. $19.75, paper.)

Charles Foster aptly describes this work as a data bank waiting to be made into a book. Sponsored by the International Association for the Evaluation of Education, its data consists of more than 30,000 responses, from sources ranging from ten-year-olds to teachers, in the Federal Republic of Germany, Finland, Iran, Ireland, Israel, Italy, the Netherlands, New Zealand, Sweden, and the United States. The resources of this international endeavor are valuable; the methodological work is useful both for its technical proficiency and the problems of multinational fieldwork and analysis. Moreover, there is an excellent review of the political socialization literature that informs those who wish to learn why "political socialization" was one of the avant garde pursuits of political science. Alas, the pathfinders are now entering middle age and the ironies of "socialization" catch up with tenured full professors. There are also nuggets for the speculative; for example, how Israel combines strong national loyalty with pervasive political cynicism and distrust of authority.

Nevertheless, this is not a book which informs us about political matters in a coherent and thematic way. Data is used repeatedly (for example, the material on participatory norms appears in no fewer than four chapters) without any theoretical foundation. The authors write their own critique in the concluding chapter:

> In spite of the fact that the transmission of civic values and attitudes (as well as the appropriate knowledge) was seen a suitable task of the school by those participating in the study, the process by which this transmission actually takes place was not clear. The methods used by the schools often seemed simply to be derived from established educational practices in the cognitive domain rather than from a thorough understanding of attitude formation grounded in socialization theory or social psychology (p. 322).

The rub is that despite skill and technique, this international effort rests upon a weak conceptual base. Scholars familiar with political socialization literature will find immense difficulties with the simplistic, rationalist premise that schools transmit values within a cognitive framework. Nowhere do the authors deal with the difficult issues of what attitudes do schools transmit? How are they communicated? What impact do schools have upon political attitudes and behavior?

Nearly a decade ago I wrote in the *American Political Science Review* that fundamental political learning, linked in concept and context to the real world, is the proper function of "political socialization" research. I am distressed that this volume has not rendered my words dated and irrelevant.

EDGAR LITT

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In the Name of the People: Prophets and Conspirators in Prerevolutionary Russia. By Adam B. Ulam. (New York: Viking, 1977. Pp. xii + 418. $15.00.)

The story of the Russian revolutionaries of the nineteenth century, the youthful men and women who forsook their aristocratic heritage to "go to the people," is the stuff of fiction. It has been told many times, often very well, in both scholarly and popular works. In English alone David Footman, Avrahm Yarmolinsky, Robert Payne, Ronald Hingley, and Eugene Lampert have written on various aspects of the subject, while the whole movement has been fully dealt with in Franco Venturi's monumental *Roots of Revolution*. Yet it still retains a compulsive fascination, so that new works are to be welcomed.

In the Name of the People, while covering much the same ground as its predecessors, adds interesting details gleaned from recent Soviet scholarship as well as older Russian works. Designed for a general audience, it is a long, absorbing narrative, written in the lively and provocative style that we have come to associate with Adam Ulam, author of previous studies of Lenin and Stalin. There are, however, serious drawbacks. Most important, the author is too much out of sympathy with his subject to treat it judiciously. Running through the book is a vein of ridicule and contempt for the "prophets and conspirators" (as Ulam styles them) who found the autocracy intolerable and were bent on overthrowing it. Ulam harbors for these Russian rebels of the 1860s the same condescension, not to say revulsion, that he harbors for their American counterparts of the 1960s, to whom he sometimes compares them. His attitude, indeed, seems one of regret that the tsarist police were not more efficient, and the courts more stringent, in dealing with the revolutionaries.

The tone of heavy irony which colors Ulam's analysis will grate on those for whom the Russian Populists rank in many instances among the noblest idealists in history. From a reading of this book one would never understand why Bakunin, Lavrov, and Chernyshevsky (whom Lampert, in *Sons Against Fathers,* has called a "saint and revolutionary") won the veneration of two generations of Russian students. To Ulam, Tsar Alexander II was "a prudent and humane reformer," whereas Anton Petrov, the leader of a peasant disturbance, was "clearly insane," Peter Zaichnevsky, who wrote the manifesto *Young Russia,* was a "young lunatic," and the terrorists Ishutin and Nechaev were of "the revolutionary Svengali type" and "clearly psychopathic." As for the rest, "puerile" is the author's favorite epithet (not to mention "deranged," "deluded," "petulant," "immature"), as if the causes of their militancy were personal aberrations rather than tsarist intransigence and repression.

Such glib psychological speculation, however, misleads more than it explains. No one would deny that the Russian revolutionary movement included a variety of human types, the sinister and base alongside the noble and humane. Yet the Populists, in the main, were not mere utopian dreamers or psychological "misfits" (another Ulam epithet) who cultivated violence for its own sake or to satisfy a lust for adventure. Ulam, however, concerned as he is with their drives and fixations, their fantasies and hallucinations, is scarcely touched by the moral tenacity and passionate humanity of these dedicated young men and women.

From historians like Footman and Venturi we get a different and truer picture. In contrast to Ulam, they succeed in capturing the spirit of idealism and self-sacrifice which alienated the Populists from the established order and in some cases drove them along the path of violence and assassination. For style and drama, Footman's *Red Prelude* remains without rival as the best study of the Populists in English; for its comprehensive scope and mastery of the sources, Venturi's *Roots of Revolution* still surpasses any other work

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Politics of the Indian Ocean Region: The Balances of Power. By Ferenc A. Vali. (New York: Free Press, 1976. Pp. xv + 272. $14.95.)

Rarely studied in depth, the countries bordering the Indian Ocean have recently attracted the attention of scholars. Ferenc Vali gives us a "synoptic" overview of this vast region, and a valuable compendium of data that is difficult to locate in one place. The historical introduction is stimulating and rewarding and the scope of material covered, together with the appendices, makes it a useful reference book.

Inevitably, an overview of such a complex "region" will suffer from distortions. Not all readers will be convinced that any useful generalizations can be made about such a diverse far-flung area. Nor will the author's subdivisions, separating the politics of the Red Sea/Horn of Africa from that of the Persian Gulf, be persuasive to all. The relative emphasis on individual countries sometimes appears arbitrary; for example, more space is devoted to Mauritius than to Saudi Arabia.

The bird's-eye approach requires careful balancing between the common themes linking the entire area and the differences among the various regions. Vali tries to justify this by arguing that politics in the regions are discrete, hence the subdivisions; but linked, hence the overview. In this attempt he is not altogether successful. It is not clear how the evolution of politics in particular regions will affect Indian Ocean politics, and whether, if in fact they do, some regions are more important than others. Interesting in themselves, the discussions of many of the regions are apparently unfocused and not directly relevant to the central strategic issues of the Indian Ocean. Nor is a persuasive case made for the assertion that both superpowers and regional powers recognize the "geostrategic unity of the region" (p. 40). The overview approach encourages generalizations that lead Vali to repeat that the U.S. has no direct vital national security interest in the "region," thereby equating U.S. interests in Persian Gulf oil with her rather more general interests elsewhere in the area.

The volume is not enhanced by the author's *tour d'horizon* which spreads the available wisdom rather thin, and results in sweeping, inconsistent statements such as, "most of the littoral states . . . would like to eliminate all non-regional navies" (p. 59), and "only a few [states] favor the complete removal of all outsiders" (p. 231). In addition to platitudes ("Kenya," we are told, "favors disarmament and therefore voted for the Zone of Peace" resolution in the United Nations [p. 141]), we are also subjected to gratuitous comments (e.g., on the 1973 oil price increase [pp. 210–19]). Vali is also inconsistent in his discussion of Iran's prompt support for the 1971 Zone of Peace resolution, which has as one of its components, denuclearization. Tehran's support for this and the concept of a Middle East Nuclear Free Zone antedated India's Nuclear explosion of May, 1974 (pp. 37, 63, 238).

The book is strikingly uneven in quality. The parts relating to the historical antecedents, and the strategic and legal issues, are good. But the author's touch is far less sure when dealing with regional politics, especially those of the Persian Gulf and Indian subcontinent. Apart from poor proofreading (the regime in Aden is the People's Democratic Republic of Yemen, pp. 137–38), the volume is marred by an eclectic bibliography. Many available relevant texts and articles are not even cited; and some sources cited were clearly not used. Consequently, there is too much reliance on undigested newspaper accounts.

Nevertheless, the author's analysis of basic trends in the region is sound. The evolution of regional (including internal) politics is likely to condition the environment affecting external powers' involvement in the area. The limits of superpower influence are also realistically underscored. But the changed nature of power in international relations and the growing separation of issue-areas in alliance relationships are only dimly glimpsed in this volume. Perhaps this accounts for the failure to discuss adequately regional and subregional approaches to security. But such an approach in any case would have undermined the synoptic view that Vali sought.

SHAHRAM CHUBIN

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Looking at the Afrikaner Today. Edited by Hendrik W. van der Merwe. (Cape Town: Tafelberg, 1975. Pp. 124. R7.20, paper.)

White South African Elites. By Hendrik W. van der Merwe, M. J. Ashley, N. C. J. Charton, and B. J. Huber. (Cape Town: Juta, 1974. Pp. 190. R6.00.)

Occupational and Social Change among Coloured People in South Africa. Edited by Hendrik W. van der Merwe and C. J. Groenewald. (London: Rex Collings, 1976. Pp. 278. R6.00, paper.)

Student Perspectives on South Africa. Edited by Hendrik W. van der Merwe and David Welsh. (London: Rex Collings, 1972. Pp. 229. £1.50, paper.)

Labour Perspectives on South Africa. Edited by Wolfgang H. Thomas. (Cape Town: David Philip, 1974. Pp. 259. R5.40, cloth; R3.00, paper.)

Anyone who believes that there is some simple formula to understanding South Africa need only read the impressive series of manuscripts brought out by the Centre for Intergroup Studies at the University of Cape Town. The bold picture of blacks rigidly separated from whites, the heavy hand of repression—so apparent to the outside world—yield in these manuscritps to nuance, ambiguity, and multiple social conflicts. The Center's research brings out the social and political gulf between English and Afrikaans-speaking South Africans (*White South African Elites* and *Looking at the Afrikaner Today*); the peculiar, perhaps anomalous position of the coloured popularion, or mixed-race group (*Occupational and Social Change Among Coloured People in South Africa*); the frustration and poverty or unorganized African workers and the grappling of a labor movement divided against itself (*Labour Perspectives on South Africa*); and an educational framework that aims to produce partisan—or at the very least, compliant—citizens, that unwittingly produces apartheid's most liberal and radical critics (*Student Perspectives on South Africa*).

The Centre's publications reach this complexity without emphasizing the bizarre or exaggerated qualities of South African life. Instead, these publications draw upon some of the best traditions in comparative politics and sociology, social science and empirical methodology. Elite attitudes, for example, are considered in light of similar work by Hadley Cantril in Brazil, Cuba, Israel, the United States and elsewhere. The consideration of the South African labor movement is prefaced by useful articles on black-white relations in American trade unions and the role of labor migrants in German labor organizations. Labor statistics for the coloured population are enlivened and made comprehensible by comparative articles on the Natal Indians and American blacks and by sound theoretical introductions by Leonard Broom, Lawrence Schlemmer, and W. H. Thomas. As a guide to the Byzantine politics among student organizations, Seymour Martin Lipset provides a useful introduction on the contradictions and tensions in modern educational institutions.

This mixture of comparative methodology and observation makes this collection essential reading in the study of South African politics and society. There is a scope and perspective in these works too often lacking in South African research. We may maintain this impression of the Centre's research and publications, even while admitting that the books and the articles within them vary enormously in quality. That range is not particularly surprising: the workshop format hardly lends itself to coherence and tight quality control. That there is anything here worth reading is testimony to judicious editing and selection of workshop participants. But, I must confess, some of the articles and certainly one of the publications should be avoided.

Labour Perspectives on South Africa is far and away the best book in the collection. In January, 1973, the Centre for Intergroup Studies and the Trade Union Council of South Africa (TUCSA) jointly sponsored the Centre's third workshop. At the height of the African strikes in Durban, they assembled an impressive body of participants from the academic community, organized labor, and management. They were not swamped by events, however. The workshop made constructive use of comparative material from the United States, Germany, and Rhodesia (unfortunately, the latter paper did not appear in the final publication). The workshop papers were influenced by an extraordinarily lucid background article by Jeffrey Lever. He sets out the prevailing fears of white workers (wage undercutting, job competition, and power rivalry), the types of protectionist strategies (unionist, e.g., "rate for the job" and separationist, e.g., "job reservation"), and the consequences of such fears and strategies for African workers. Other background papers on industrial legislation, labor force characteristics, and the changing wage and occupational structure provide a comprehensive picture of the labor market and opportunities for labor organization. The quality of the background articles are matched by a provocative interchange between the trade unionists themselves. The workshop successfully assembles some of the most flamboyant and informed trade union leaders in South Africa. They introduce a sense of reality and history and give meaning to the choices set out at the beginning of the book. Anna Scheepers tells the story of a garment workers union that sought to incorporate the unorganized coloured and African factory workers; T. C. Rutherford describes the highly skilled typographical workers union that acted like skilled unions elsewhere in the world; and J. H. Liebenberg tells of the skilled railway workers union and the Confederation of Labor that formally barred membership by nonwhite workers. The coherence and scope of this book are a credit to the credibility of the Centre, and TUCSA and the skill of the book's editor, Wolfgang Thomas.

Though *White South African Elites* seems to lack an organizing principle, its data analysis hammers at a single theme: that religion and language, and not class, are the primary divisions within the ruling elite. Broad differences emerge between the English and Afrikaans-speaking leadership on socialization, attitudes toward segregation, the polity, democracy and virtually everything else. What they do agree on is the Afrikaner's preeminence in the political sphere and the use that has been made of the preeminence as a vehicle for Afrikaner social and economic advancement. The English leaders, dominant in the economic sphere, appear as political invalids, slightly apathetic and awed by the Afrikaners' seemingly unshakeable hold on political authority. Though there are good reasons to question this analysis, the argument is made persuasively and, unlike much of the debate on the question, informed by credible data. Those, like myself, who want to emphasize class differentiation within the ruling elite, now must assume the burden of proof and an obligation to employ systematic data collection.

The survey method among elites now has a legitimacy in the study of South Africa that it has long enjoyed in the United States and Europe. The work of Hendrik van der Merwe and his colleagues stands alongside similar work by Heribert Adam and, I would like to think, Stanley Greenberg, as a body of data that must be incorporated into the ongoing scholarly debate. The difficulty with *White South African Elites* is not the method or approach but the tendency to aggregate data in a way that obscures its richness. After arguing that the leadership structure in South Africa is highly differentiated, the book considers the elites *as a whole*. The lumping together of clearly differentiated elites makes mush of provocative data on the various sectors of labor, industry, government, and religion. Unfortunately, the pressure of publishers (this manuscript was reduced to half its original size) forces these compromises on all of us.

The fifth workshop of the Centre took on the always difficult and intricate problems of the South African coloured population. The coloureds have lived with particularly onerous forms of steadily tightening racial exclusion, from the narrowing and elimination of their franchise rights to the disruptions of the Group Areas Act. Nonetheless, the coloureds occupy a middle position and reap the benefits as well as the disabilities that follow from it. While *Occupational and Social Change Among Coloured People in South Africa* lacks any vital or new contribution to the question, the collection as a whole underlines the emerging stratification in the coloured group. Leonard Broom and William Beinart's analyses, for example, outline a seeming inexorable trend: the gradual movement of the coloureds into white-collar and entrepreneurial roles (though not upper-level management or large-scale ownership) and the increasing importance of coloureds in the labor movement. Sheila T. van der Horst describes how such key apartheid institutions as job reservation and influx control help protect coloured workers against the competition of more plentiful and lower-paid African workers. The middle and paradoxical situation of the coloureds in South Africa is made no less perplexing by this book. But the reality and importance of their position is given new meaning.

The first workshop in January, 1971, took up the question of South Africa's universities and student organizations. The book that emerged from the workshop is by no means analytical or historical. Lipset's introduction and David Welsh's essay on university life and social structure provide a nice context for considering what are essentially documents on the problem. The full range of student interests and politics find spokesmen in these pages, from Johan Fick's assault on NUSAS's "neo-liberalism," Neville Curtis and Clive Keegan's commitment to "free education in a free society," to Steve Biko's rejection of whites in the black man's struggle. Perhaps more interesting in light of events in Soweto is the evidence of inflated aspirations among African high school pupils. These essays add up to an elaborate commentary on the contradictions in the South African educational system. The introductory material provides some enticing ideas on the meaning of it all, but for the most part, one must provide that meaning for oneself. Judging by the upheavals of recent years—the Durban strikes of 1973 and 1974 as well as the Soweto disturbances of 1976—the need to find that meaning is no less important now than it was at the time of the first workshop.

Looking at the Afrikaner Today is probably the weakest link in the Centre's publications. Herman Giliomee's essay on "The Development of the Afrikaner's Self-Concept" is clearly the best essay in this work and among the best in the entire Centre collection. His article successfully demystifies the Great Trek and shows how group consciousness emerges through the vicissitudes of nineteenth- and twentieth-century life. The division of Afrikaner identification into notions of volk, white population group, cultural groups and plurality of associations lends meaning to the dilemmas and conflicts in modern Afrikaner thinking. Unfortunately, the remaining essays fail to live up to Giliomee's challenge. They are impressionistic and unsubstantial, adding little to our understanding of the thinking and condition of perhaps the dominant ethnic group in South Africa.

The Centre for Intergroup Studies has brought out an impressive collection of works on South African social and political life. They vary in quality and coherence, as we might expect from publications based on the workshop format. But there are individual articles and books here that meet the highest standards of scholarship. The collection as a whole provides important insights into the social conflicts and contradictions that plague South African society. We owe a debt of gratitude to Hendrik van der Merwe for raising important questions and for providing a forum where scholars and political actors can meet and make sense of a sometimes obscure reality.

STANLEY B. GREENBERG

Yale University

Peasants into Frenchmen: The Modernization of Rural France, 1870–1914. By Eugen Weber. (Stanford, Calif.: Stanford University Press, 1976. Pp. xv + 615. $20.00.)

This book is a treasure trove, each page glittering with nuggets of detail documenting Weber's subject: the formation of cultural unity in France between the overthrow of the Second Empire and the outbreak of the First World War. In 1876, a student at a Teacher's College in Limoges couldn't say more than two words about Joan of Arc (p. 111); as late as 1906, only one military conscript in four could explain why July 14 was a national holiday (p. 110). When the Marseillaise was written, most Marseillais barely spoke French (p. 439). At the time of the Third Republic's founding, French was a foreign language for over one-quarter of the population (p. 67); by 1900, the young everywhere were using it in preference to their local patois, though in some areas the final conquest by French did not come until the war.

Weber provides fascinating material on less familiar dimensions of national integration than language and shared history: Fear and security: wolves still threatened human beings and sheep till the end of the century; 1,316 bounties were paid in 1883, only 115 in 1900 (p. 15). Popular belief: in 1840, all but 14 out of 500 parishes in Gers rang church bells when storms threatened; in 1956, 143 out of 337 were stil ringing the *carillon de tonnerre* (p. 28). Measurement: the common use of the metric system took far longer to take root than the Nouveau Franc. Fêtes: these were restructured: less religious, more secular; less tied to the critical moments of the year (such as the harvest), or of life (such as marriage), more dispersed to be convenient as holidays. As fêtes were abandoned, the composition of their celebrants changed: from young men to children, from rich to poor, as in the case of New Year's singing masquerades (p. 392). "Veillées" (evening gatherings in the barn or someone's hut in order to work, socialize, save fuel and light, Ch. 24) and "charivaris" (ritual, or not so ritual, punishments of various transgressors against local custom, Ch. 22)–gradually died out, replaced by cafes and courts. Oral legends were driven out by printing, from French song sheets which replaced native tunes, words and themes, to the penny press, schoolbooks, newspapers (Chs. 25–22).

The agencies of change are what one would expect: transportation (especially the railroad), schools, military conscription, migration, technology, market forces, increasing division of labor, politics. These produced national integration, Weber argues. The impact of 1789 and 1848 was not trivial, just incomplete. Politics in rural France remained archaic (meaning local and personal) until the 1880s; after that, gradually, local interests "seem" to become inextricably linked to national and international ones, and understood as such (p. 241).

The great contribution of this book is not theoretical but empirical. The range of materials, the documentation, the feel for language and meaning are all awesome. The last chapter contains interesting comments on theories of integration and colonialism (Deutsch, Fanon) as applied to metropolitan France itself. The recent resurgence of peripheral nationalisms in Europe bring out the importance of Weber's work, which reminds us how recent present levels of cultural integration are, even for France, where this revival is relatively weak. Integration is never total. Old bases of cleavage can revive, and new ones emerge. Political conflict does require integration (p. 486) and can promote it, but politics can also provoke profound cultural divisions. The socializations undertaken by Catholic, lay, Communist, Socialist, and conservative networks in contemporary France make for rather different Frenchmen with divergent understandings of society, history, work, morals, human relations, and the future, as do the division of labor, economic differentiation across different regions, and other economic forces. What Weber shows is that very significant transformations did occur between 1870 and 1914. Whatever the importance of new distinctions, an extraordinarily complex array of old ones dissolved. No one interested in the processes of integration in Europe, and France in particular, should fail to examine this book. It is a worthy complement to Roger Thabault's *Mon village*, which inspired it.

PETER GOUREVITCH

McGill University

Electoral Politics in the Indian States. Edited by Myron Weiner and John Osgood Field. 4 Volumes. Vol. 1: *The Communist Parties of West Bengal.* By John Osgood Field and Marcus F. Franda. 1974. Pp. xvii + 158. $9.00. Vol. 2: *Three Disadvantaged Sectors.* By Jagdish N. Bhagwati, Padma Desai, John Osgood Field, William L. Richter, and Myron Weiner. 1975. Pp. xxii + 199. $11.00. Vol. 3: *The Impact of Modernization.* By John Osgood Field, Francine Frankel, Mary F. Katzenstein, and Myron Weiner. 1977. Pp. xx + 193. $13.00.) Vol. 4: *Party Systems and Cleavages.* By Marguerite Ross Barnett, Craig Baxter, Paul R. Brass, Robert Hammond, Robert L. Hardgrave, Jr., and Glynn Wood. 1975. Pp. xxiii + 204. $13.00. (Delhi: Manohar Book Service, distributed in the U.S. by South Asia Books.)

Sponsored by MIT's center for International Studies, this collection includes 12 analyses of electoral behavior in India. The individual contributions are addressed to three major subjects: the relationship between modernization and electoral behavior, the politicization of socio-ethnic cleavages, and the institutionalization of support for political parties. The first of these themes is sounded in analyses of urban constituencies (Weiner and Field), migrant political behavior (Katzenstein), and the electoral consequences of the "green revolution" in Uttar Pradesh (Frankel). The effects of modernization are also assessed in analyses of three "backward sectors"—areas that were formerly princely states (Richter), tribal constituencies (Weiner and Field) and women candidates (Desai and Bhagwati). The electoral consequences of cleavages are investigated using religion in the Punjab (Brass), cultural nationalism in Tamilnadu (Barnett) and caste rivalries in Karnataka (Wood and Hammond). Finally, institutionalization of electoral support is examined with respect to the communist parties of West Bengal (Fanda and Field) and Kerala (Hardgrave), and the short-lived BKD in Uttar Pradesh (Baxter).

Myron Weiner and John Field test two models of urban politicization. The "attributional" model posits urban uniformity whereas the "contextual" model allows for urban-rural interaction, thus producing variation in urban electoral behavior. The two models are found to "coexist." Over time urban constituencies are becoming distinctive with regard to electoral competitiveness and support for radical parties. Urban-rural differences in level of participation, however, have been reduced.

Mary Katzenstein's examination of urban migration notes the need to decompose the migrant experience into more specific traits. The two most important traits are found to be length of time the migrant has lived in the city and the degree to which the migrant is committed to urban residence. These two variables are associated with increases in urban turnout. In keeping with the Weiner/Field paper, Katzenstein also notes that the determinants of migrant political behavior are far from uniform. In "backward" states, for example, the percent of migrants from rural areas is inversely correlated with urban turnout. In other states, place of origin is independent of turnout.

Francine Frankel examines the electoral consequences of agricultural modernization in Uttar Pradesh by dividing the state into economically homogeneous regions and then correlating indicators of agricultural modernization with electoral behavior in each region. As expected, the results vary across region. In the "green revolution districts," agricultural modernization has weakened ascriptive and communal bases of party support and strengthened economic class-oriented loyalties. While support for the BKD, a populist, peasant party is positively correlated with agricultural modernization, Congress and Jan Sangh support varies inversely with modernization.

William Richter's analysis of differences between princely and non-princely districts reveals that the former are not significantly less developed. Participation rates, however, are lower in the princely constituencies, as is the extent of Congress support. These differences are attributed to "divergences in political socialization" which stem from the more limited opportunities for political participation in the princely states. It should be noted that two of the major princely states (Hyderabad and Travancore-Cochin) are excluded from consideration.

Weiner and Field are concerned with tribal voting behavior. Turnout is lower among tribal constituencies and over time the "participation gap" between tribal and non-tribal constituencies has increased. The contextual model also applies to tribal electoral behavior as tribal constituencies are more likely to resemble non-tribal constituencies from the same state than tribal constituencies from other states.

The final article dealing with a backward sector is Padma Desai's and Jagdish Bhagwati's study of women candidates from the Lok Sabha and state legislative assemblies. Among the political parties, Congress is the most receptive to women. Once nominated, women are more likely to win than men, leading the authors to conclude that "given the discrimination against women, . . . women candidates are likely to be more 'gifted' and hence are more likely to win than men candidates" (p. 174). While there can be no denying that some women candidates are "gifted," an alternate explanation may be that safe seats are earmarked for women. Congress with its abundant supply of such seats is thus able to run more women than the opposition parties.

A principal feature of India's political development has been the gradual transformation of traditional identifications into current political loyalties. Paul Brass convincingly demonstrates the linkage between religion and party support in Punjab. Politics in Punjab is found to be a variant of the "consociational democracy" model in that religious and ethnic cleavages are moderated through elaborate networks of communal coalitions. The party system is characterized by competition between Congress, which attempts to build coalitions across communal lines (coalitions which tend to be extremely unstable), and the Akali Dal, a party which appeals exclusively to Sikhs.

The rise of the DMK in Tamilnadu, from the days of the Justice Party to its emergence as the dominant party in the state, is described by Marguerite Barnett. She traces the gradual shift in the party's ideology from anti-Brahminism to an anti-northern, anti-Hindi posture. The recent split in the party and the resulting formation of the ADMK is given very brief treatment.

Wood and Hammond examine the unexpected success of Mrs. Gandhi's New Congress in the 1971 Karnataka Lok Sabha elections. Their analysis does not support the caste conflict thesis, which explains Old Congress losses as caused by opposition to the Lingayats, who dominated the party. They show that the New Congress was able to win by retaining the support of constituencies previously controlled by the undivided Congress. While the electoral pattern is one of continuity, a dramatic "changing of the guard" is evident in 1971. The leadership of the New Congress (the Devaraj Urs Ministry) represents a new generation of Mysore politicians; they are less likely to be lawyers, are younger and more rural in origin.

The remaining papers are concerned with patterns of electoral support for individual political parties. In Kerala and West Bengal region is an important determinant of communist support. Following the split in the Indian communist movement, the CPM has emerged as the stronger of the two parties in both states. Coalition formation in both states is traced to the communist split. In both states, CPI has been able to function as a swing party capable of forming a majority with Congress. In Kerala, both parties have participated in numerous "Fronts" and "Mini-Fronts" most of which have been unstable. (In this regard, Hardgrave's article contains a detailed discussion of the parties, issues and personalities underlying political coalitions in Kerala.) Finally, Baxter traces the rise and fall of the BKD, a "flash" party which gained 98 seats in the UP legislative assembly in 1969. Extreme factionalism within Congress and ethnic group rivalries account for the instability of electoral support in UP.

This collection is certainly of immense value to students of Indian politics. The entire set of data (spanning the 1952 and 1971 General Elections) has been made available to the major archives so as to enable further analysis. The most impressive aspect of the individual contributions is the combination of familiarity with the Indian cultural context and sensitivity to methodological problems in the use of aggregate data. The authors are not prone to carelessly generalize their results to the all-India level. For those interested in comparative electoral behavior, however, the work is of limited relevance. Many of the papers are atheoretical in orientation and are more concerned with describing electoral patterns in one Indian state than with testing propositions derived from the political parties or voting behavior literature. There is no attempt, for example, to investigate the relationship between electoral support (votes) and legislative representation (seats). Similarly, the articles which deal with coalition formation ignore the models developed by Riker, Gamson and others. On the methodological side, most of the authors avoid use of multivariate analysis. The reader is thus not in a position to assess the relative importance of economic variables and social cleavages as determinants of electoral behavior. While there is not much explanatory/predictive analysis, the reader is deluged with data. The four volumes feature 51 tables, 38 figures and 294 pages of appendices.

SHANTO IYENGAR

Kansas State University

Contemporary Mexico: Papers of The IV International Congress of Mexican History. Edited by James W. Wilkie, Michael C. Meyer, and Edna Monzón de Wilkie. (Berkeley: University of California Press, 1976. Pp. xv + 858. $27.50, cloth; $12.00, paper.)

The paradoxical title of this book is suggestive of the broad range of topics included in it. They concern both contemporary Mexico and its past, ranging across all aspects of Mexican life, its politics, literature, education and religion, to cite just a few. Because of the multiplicity of the subjects covered it is quite difficult to determine in what new directions Mexican studies is headed. No one area is covered in any depth; consequently, the scholar is given little assistance in determining what areas should be investigated, what new conclusions or theories should be attacked or supported, what research lacunae remain. Perhaps because of this shortcoming, the reader is promised that the upcoming Fifth Congress is to be oriented around a single research topic, "Labor and the Working People."

Despite the general criticism made above, however, it is possible to draw a few tentative conclusions concerning some aspects of Mexican studies, especially as they relate to areas of interest to the political scientist. One of these involves the future direction of the Mexican political system, an issue which did not receive as much attention as it probably deserves. Roger Hansen's argument that Mexico's governing elite should be able to maintain the status quo is an interesting departure from more pessimistic views which foresee the system's imminent collapse. Hansen's paper, however, does not go much beyond his excellent analysis in *The Politics of Mexican Development*.

One article which is particularly distressing due to its surprising naiveté is Martin C. Needler's. In a throw-back to the most ethnocentric developmental literature of a decade ago, Needler argues that since the Mexican regime has helped to bring about development it must be judged by the standards of the developed countries. He fails, of course, to realize that different countries, and different individuals within those countries, might have different ideas as to what constitutes development. This same general criticism could be applied to Needler's statement that since the Mexican government has brought that country out of the nineteenth century, it should be examined in light of the practices and beliefs of the twentieth. This is surprising since Needler's standard of development is liberal democracy which–it could be argued–was most characteristic of the last century while totalitarianism and authoritarianism were more typical of the present century.

What is lacking, however, in analyses of the present and future shape of the Mexican political system is largely compensated for by the thought-provoking interpretations of the development and historical significance of revolutionary Mexico. In a section devoted to the periodization of Mexican history, Michaels and Bernstein attempt to divide Mexican history (beginning with Porfirio Díaz' accession to power) into different eras using as the major criterion the development of the Mexican bourgeoisie. On the whole it is a rather orthodox account. The authors' assessment of the Cárdenas period is, however, somewhat different and deserves greater elaboration. Absent is the image of the Cárdenas group as the heroic defenders of the people, trying to forge a new progressive order in Mexico, one in keeping with the betrayed ideals of the revolution of 1910. Instead, the *cardenistas* are described as a counterelite which used the widespread disillusionment with laissez-faire capitalism to seize power. The goal of Cárdenas' supporters allegedly was that of "modernizing the free enterprise capitalist system in order to better insure its survival (and that of their privileges)" (p. 701). The authors further argue that in reorganizing the official party, Cárdenas did not democratize the party but rather strengthened its authoritarian characteristics. This differs from the more traditional interpretation which suggests that Cárdenas wanted to organize the masses so that they could better defend their own interests but that his successors turned the party structure on its head and used the sectoral organization to control the peasant and urban working-class masses.

Also of interest is Michaels and Bernstein's denomination of the immediate post-Cárdenas period from 1946 to 1958 as the "Porfiriato Restored." This conceptualization of the outcome of the Mexican Revolution appears to be in agreement with the thrust of the argument presented by Jean Meyer in his paper on periodization. Meyer emphasizes the continuity between the *Porfiriato* and the Mexican Revolution. He sees both Díaz and Calles, together, as giving Mexico its current political and institutional structure. The revolution is seen as a modernized version of the *Porfiriato* which has succeeded in strengthening the state. This line of reasoning is also similar to that advanced by Hansen in *The Politics of Mexican Development*. It thus appears that a new orthodoxy in the interpretation of Mexican political evolution may be shaping up, one which sees the Mexican Revolution not so much as a turning point, but rather as the beginning of another stage in the development of Mexican life, one which preserves many of Mexico's age-old customs and traditions–such as the dominance of authoritarian relationships–but which uses them to create a strong centralized state and a modern, growing economy.

Finally, there are two articles which should be singled out for special attention. The article presented by John and Susan Purcell is a stimulating attempt to compare the functioning of the PRI with the machine politics of U.S. cities. The differences which they find between the two are also of interest since they suggest why the PRI should be able to survive economic development while the U.S. urban machines did not. Perhaps the most significant paper is Peter Smith's study of political elite mobility. Methodologically, his is the kind of practical empirical research which should be encouraged. Substantively, it lends credence to the theory that the current stability of the Mexican system is due to the high level of elite circulation, something which did not exist in the Porfirian regime and which helped contribute to its collapse.

FELIX G. BONI

Chestnut Hill College

The Governance of Britain. By Harold Wilson. (New York: Harper & Row, 1977. Pp. xiii + 219. $10.95.)

Despite what the title implies, this is not a general study of the British political system; rather it is an examination of the role of the prime minister. No one has served longer as prime minister in peacetime during this century than Harold Wilson and only Asquith and Churchill exceeded him in total service. Such a book by such an author cannot be without significance. Anyone who expects, however, to find in it inside information about the prime minister's part in key decisions of the last decade will be disappointed.

Nonetheless, this book has its virtues. It is informative; it provides some interesting anecdotes without becoming anecdotal. It shows good knowledge of the relevant academic studies and combines some of their principal points with the author's personal experience to offer a coherent picture of the prime minster's role. Sir Harold wisely organizes his material topically rather than chronologically, thereby managing something of an analytical approach. Thus someone wanting to study thoroughly the prime minister's office would find this book a useful starting point. Furthermore, the writing is enlivened here and there with flashes of the biting wit which fondly recall the Wilson parliamentary performances of the "thirteen wasted years." And while self-serving comment has not been avoided entirely, it has been held to a minimum. Had this book been written by anyone else, it could have been welcomed as a sound, useful contribution; coming from someone who was prime minister for almost eight years, there could have been, should have been, so much more.

Sir Harold offers a brief history of the development of the position of prime minister and then discusses the process of selecting cabinet colleagues. He describes the procedures and activities of cabinet meetings. He explains the organizational structure of his personal staff and how this was distinct from, but related to the Cabinet Office. Managing Parliament and party (primarily PLP and NEC) are each given a chapter. The prime minister's duties as First Lord of the Treasury and in national security matters as well as his role in making foreign and economic policy are covered. These are surprisingly dull chapters, characterized by legalistic descriptions or boring accounts of meetings.

The concluding chapter offers a number of pedestrian comparisons of the prime minister with the American president, which will be familiar to any undergraduate who has taken a British politics course. The only interesting comment is Wilson's claim that "most British governments are extremely wary about taking action in the year before a presidential election which might influence voters' attitudes" (pp. 175–76). In 1964 Sir Harold even went so far as to tell his colleagues not to create any problems during the six months prior to the midterm congressional elections. And some people question whether there is "a special relationship!"

Among the more informative portions of the book is the detailed discussion of question time. Sir Harold explains the tricks used by MPs to waylay the prime minister with supplementaries (e.g., the do-you-intend-to-visit? question) and the means used by a prime minister to protect himself. He also discusses some of the potential abuses of question time that have come to light recently.

Sir Harold spends some time defending himself against the charges of excessive or improper use of political advisors. The most informative aspect of this discussion is its clarification of the role of the Policy Unit at No. 10 as distinct from the Political Office and the Private Office as well as from the Central Policy Review Staff and the Cabinet Office Units in the Cabinet Office.

While these and other similar topics are interesting, the most significant theme of the book is Sir Harold's explicit rejection of the Mackintosh-Crossman thesis of prime ministerial government—this despite the fact that he grants that the power of the prime minister has grown significantly for a variety of reasons, with its apogee reached under Harold Macmillan. It is a nice question whether the evidence presented in this book does more to support than to reject Sir Harold's position. For example, while he has some good words to say about consultation in constructing a government and in deciding to call an election, it is clear that *he* decided. He dismisses the "power" of dissolution with the comment that a prime minister who believed that this could serve as a weapon to control dissidents would be "certifiable" (p. 40). On the other hand, in what sounds like a modern version of *The Prince,* he advises prime ministers "to insure that there is no single crown prince. I never had less than three or four, and at one time—it happened to be crucial—six" (p. 35). Until he became prime minister in 1964, ministers who disagreed with a cabinet decision, but did not feel deeply enough to resign, could have their dissent recorded in the official records. Sir Harold refused to permit this and instead suggested that such ministers send him an official minute. Subsequent ministerial inquiries spread over the following decade about the fate of these communications have established only that they are " 'entered in a register,' as in fact are all minutes on all subjects" (p. 59). But he asserts, although he never could get Richard Crossman to believe it fully, that he did not direct the cabinet secretary's writing of the conclusions so as to alter the cabinet's decisions to conform more closely to his own preferences. He maintains that not only did he not see drafts before the conclusions were circulated, he rarely even bothered to read them afterward.

Despite the limited new information which this book offers, it cannot but help to contribute to the continuing debate about the locus of power in the British political system. To accept this account, however, as necessarily the most valid and insightful, in view of the author's background, would be an error.

JORGEN S. RASMUSSEN

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The Politics of Cultural Pluralism. By Crawford Young. (Madison: University of Wisconsin Press, 1976. Pp. xii + 560. $20.00.)

Like other scholars of contemporary politics, Crawford Young was struck by the strength of cultural subnationalism in the politics of the developing world. In part, this recognition grew out of his field experience in the ethnically turbulent Congo (now Zaire). In part, it grew out of his recognition of the intellectual anomaly that cultural pluralism posed for his discipline. Most of what was known and thought in the early studies of developmental politics created the expectation that parochial allegiances would erode or disappear. Obviously, they did neither. In this study, Young seeks to document the variety of forms which subnational identities have taken and to account for their strength and variety by examining their origins in the political and social processes of the developing nations.

The study has many strengths. While earlier works on the subject have shown notable diligence in their accounts of varieties of pluralistic politics—Rabushka and Shepsle's *Politics in Plural Societies* would be an example—none that I am aware of matches the range and sense of command exhibited by this study. Scholars with a wide variety of research objectives could profitably consult the case studies contained in this volume. In addition, scholars will learn much from their imaginative juxtaposition—Tanzania vs. Uganda, Nigeria vs. India, and Indonesia vs. the Philippines, for example. Recent events have obviously overtaken some of the case materials, most notably those drawn from India. Nonetheless, readers will find insights that survive even fundamental political changes.

Young's coverage of the intellectual foundations for the study of developmental politics wins added respect. Intellectual history, as in his analysis of the concept of state and nation; comparative history; anthropology, sociology and social psychology—these traditionally lie at the basis of the study of political development and Young analyzes them thoughtfully and well. To many scholars, Young's formulations will mark him as a member of the "old school" of development research. The work is not grounded on more recent additions to the discipline: theories of rational behavior and social choice and methods of statistical inference. (The one exception would be in his analysis of Nigerian politics; but there, his use of concepts from game theory is largely heuristic.) Scholars would be making a major error to shy away from Young's study upon noting the era from which his approach derives; the "old school" has much to teach them. The value of this particular work is not bounded by its membership in a particular intellectual grouping.

While working within the "grand tradition" of developmental politics, Young departs from his predecessors in at least two significant ways. He gives much greater weight to explicitly political factors. In particular, in his handling of the dynamic nature of cultural identifications, Young emphasizes the impact of what he calls "the political arena": the structure of political institutions and the configuration of politically relevant groups. These, he contends, determine in large part the particular form of cultural politics that arise in the developing nations. In addition, Young uses, and takes advantage of, a more broadly comparative perspective than did most of his predecessors.

In sum, this is a work of learning drawing upon a vast array of empirical methods and thoroughly and knowledgeably grounded in the intellectual tradition of the discipline. It is an impressive work of scholarship.

The book's reputation will clearly withstand debate, and I do have several reservations about it. Where I would most fundamentally disagree with Young is in his tendency to underplay the role of material interests. He believes "states" do things; I find it more congenial to regard the state as a set of institutions through which people pursue their particular interests. He discusses "socialism" as if it were an independent doctrine. Socialism can also be viewed as a set of ideas which tends to be advocated by those who would benefit from the redistribution of income or the reassignment of property rights and which tends to be opposed by those who would lose from such measures. Moreover, the passion of cultural identities apparently tends to be most intensely experienced by those who have the most to gain or lose in a material sense. Even the social psychologists whose studies of stereotyping Young uses so well related the rise of *political* discrimination against blacks in the South and Orientals in the Western United States to economic fluctuations. Young is a careful scholar; he too is sensitive to the material basis of much of what he studies. I simply would have grounded more of the analysis upon such considerations and would have more thoroughly pursued the implications of such a perspective.

In addition, while it would seem unfair to urge Young to include further comparative materials, the emergence of ethnicity is, as he himself notes, not confined to the developing nations. This fact might suggest that the examination of cases from the developed world would be in order; and it certainly raises fundamental questions concerning the relationship between the rise of cultural pluralism and the processes of modernization and development—e.g., such as whether the last two factors have any relevance at all. Lastly, there are times in this study when the reader wishes that Young would put aside his relentless presentation of his materials and simply and clearly articulate his reasoning and his conclusions.

These points aside, there can be no question but that this book will win the admiration and respect of all serious scholars of comparative politics.

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International Politics

Ghana and Nigeria 1957–70: A Study in Inter-African Discord. By Olajide Aluko. (New York: Barnes and Noble, Harper and Row, 1976. Pp. 275. $17.50.)

The stated purpose of the book is to find out why the great expectations of cordial relationships and fruitful cooperation between Ghana and Nigeria did not materialize. The author asks a number of questions: (1) What were the main sources of disharmony in Ghanian-Nigerian relationships? (2) Were they caused by "the dark forces of imperialism?" (3) Were the sources of discord more fundamental? If so, what were they and how could they be explained? (4) Under present circumstances, what are the prospects for cooperation and cordial relationships, given the fact that both countries are currently under military regimes?

The author stipulates that his chief concern is not with passing judgment. However, he admits that it is difficult to avoid judgments completely. He assures his reader that where judgment is passed, it is passed on the basis of available information. Aluko uses official documents and newspapers. Between 1969–72 he conducted interviews with top-ranking officials and former politicians of Ghana and Nigeria. Aluko knows that the lack of access to diplomatic dispatches and other official papers was a handicap. Moreover, he admits that coming so soon after the events with which it deals, the book probably lacks historical perspective.

The book discusses Ghana-Nigeria relationships in two broad periods, first, the pre-1966 relationship prior to the military overthrow of the Nigerian civilian government of Sir Abubakar Tafawa Balewa, and second, the post-1966 period. Chapter 1 discusses the similar and dissimilar problems in the external environment of both countries prior to independence because the author believes that these factors not only set the context for the post-independence international interactions but also largely explain the external behavior of those countries. In accepting the interplay between the domestic and external forces, the author acknowledges his indebtedness to the work of F. S. Northedge in *The Foreign Policies of Powers.* He concludes that several factors suggest difficult times ahead for the Ghana-Nigeria relationship: (1) the vast differences in size and population of the two countries, (2) the different social backgrounds of the government elite until early 1966, (3) the differences between the structure of the governments, (4) the different roads which the two countries took to independence, and (5) the fact that the two countries are neither militarily nor economically interdependent: "They had to compete for the same market for their products and for financial and military aid." The author identifies an uncommon fact: during the second half of the 1960s the countries were moving in opposite directions. For instance, Nigeria was witnessing centralization within a federal structure while Ghana embarked upon progressive decentralization of administration. The power of tribal chiefs was being curbed in Nigeria but in Ghana the tribal chiefs were gaining power.

Nevertheless, the expectations for cordial relations continued because of the belief in (1) the potential strength of common colonial heritage, (2) vestiges of British interterritorial institutions, (3) the political, economic, and social cooperation between the four British West African colonies in pre-independence days, and (4) similar patterns of administrative, legal, and educational systems—for example, the West African Cocoa Research Institution, the West African Institute of Oil Palm Research, the West African Council for Medical Research, the West African Examination Council. Regrettably, of all of these interterritorial institutions, only the West African Examination Council survived into the post-colonial era. The failure to use these as foundations for closer cordial relationships in post-independent era derives primarily, says Aluko, from the ideological difference, political discord, bitterness arising out of the struggle for leadership in Africa, conflicting views about African unity, the Congo crises, attitudes to the great powers, and Ghana's attitude toward the Nigerian civil war.

The change in Nigerian relationships with the superpowers appeared to result from the Nigerian civil war, domestic pressure, and changes in Nigerian interests. In the case of Ghana the turning point was the overthrow of the Nkrumah regime. Here, changes in ideological orientation of post-coup leaders, economic difficulties, and isolationist desires played a key role. Chapter 7 carries an interesting discussion of the role of the Nigerian civil war in the disruption of a move towards the current Ghana-Nigeria relationships.

If there is a criticism to be leveled against the book, it is to what some may regard as a somewhat harsh treatment of Nkrumah's Ghana relative to anticolonialism, liberation struggles and support for liberation organizations. Seemingly, the facts of the case are objectively and clearly set forth. However, somehow the author failed to give adequate credit to Nkrumah. Clearly in these regards, Nkrumah—not Sir Tafawa Balewa—displayed remarkable foresight. Perhaps the personal interviews with Ghana's "top officials" and former politicians, taking place in the Ghana environment of 1969 to 1972, made it difficult for Aluko to give adequate credit to Nkrumah. On balance, though, this is a carefully researched and well-written book.

VICTOR A. OLORUNSOLA

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China Policy: Old Problems and New Challenges. By A. Doak Barnett. (Washington: The Brookings Institution, 1977. Pp. xi + 131. $8.95, cloth; $2.95, paper.)

The visit of an American president to Peking in early 1972 ended 20 years of overt hostilities between the United States and the People's Republic of China and began a period of realignment of the relations among the East Asian powers. Nonetheless, however momentous this initial visit, it represented only the beginning of what promised to be a lengthy process of normalization of relations between China and the United States.

By late 1973 and early 1974 there were signs that the normalization process had begun to slow down; as Barnett correctly points out, the relationship had entered a holding pattern. The principal obstacle to continued progress appeared to be China's dissatisfaction with the United States' continued support of the government of the Republic of China on Taiwan. Barnett contends that if the U.S. cannot continue to progress toward normalization with China, even the present level of détente may be lost—in other words, if U.S.-China relations do not get better, they are very likely to get worse. The new leadership in Peking may move to reduce Sino-Soviet tensions; if this happens while U.S.-China relations are still fragile, fear of Sino-Soviet cooperation could increase tension in East Asia and compound the difficulties of maintaining equilibrium in that region.

To avoid this eventuality, the author suggests a rapid solution for the Taiwan problem. The Japanese precedent of formal recognition of the P.R.C. while downgrading political ties with Taiwan to "nonofficial" status cannot be adopted by the U.S. because of this country's defense treaty with Taiwan; instead, Barnett advocates that the U.S. end this formal treaty while making clear that it will continue to oppose any attempts to change Taiwan's status by force. With the removal of the major obstacle of Taiwan, the way should be clear for the establishment of formal diplomatic ties between the U.S. and China. Nonetheless, Barnett cautions, recognition is an essentially symbolic act which will not automatically produce major changes in the substance of relationships. He presents a list of bilateral (trade and other economic relations; scientific, technical and cultural exchanges; military security and arms control) and multilateral (China policy in relation to the emerging four-power equilibrium in East China) issues to be worked on over the next three decades and suggests possible solutions.

The author, born in Shanghai, has had a wide range of experience in Chinese affairs, and his opinions will be taken seriously. Few will object to the issues he has raised, and the solutions he proposes are eminently reasonable. His writing is admirably concise and the book's compact length (125 pages) will recommend it to international relations specialists seeking guidelines to future policy in East Asia, and as a supplementary text for students of American diplomacy.

Despite the obvious strengths of the work, however, its basic premise is highly questionable. While Sino-American relations have indeed, as Barnett points out, remained in a holding pattern over the past several years, Sino-Soviet relations have remained static over an even longer period of time. The hostility between the CPR and the Soviet Union, not any idealistic desire for friendship and mutual accommodation among peoples, is what led Chinese leaders to seek rapprochement with the United States. The Sino-Soviet confrontation appears to be based on fundamental issues which are not conducive to easy solution. Rejection of the USSR's condolence note on the death of Mao was among the first public acts of the new Chinese leadership, and they have continued to single out the Soviet Union as China's number one enemy. Thus the likelihood of a marked deterioration in Sino-American relations is relatively small.

It would certainly be desirable to improve relations between China and the U.S. whether or not one believes that relations between the two states will deteriorate otherwise, but any actions intended to improve these relations should be based on a careful calculation of gains and losses for U.S. interests as well as for the morality of these moves. It is abundantly clear that the overwhelming majority of Taiwan's 16 million people do not wish to live under a communist government. To weaken the international legitimacy of the Taipei government by derecognizing it in order to obtain full diplomatic relations with Peking would be a denial of reality akin to United States' attempts during the previous 20 years to pretend that the Peking government did not exist, and moreover it would be a move which would be without tangible benefits to U.S. interests.

As Barnett points out, the Sino-American rapproachement thus far has been more to Peking's benefit than to Washington's. Militarily weak China has reduced confrontation with a much stronger United States, and has gained leverage with the Soviet Union thereby. China has also been able to purchase urgently needed commodities such as grain from the U.S. while, as Barnett admits (p. 36), the establishment of these economic ties is and will continue to be of minor importance to the U.S. Exchange programs have enabled Chinese visitors to acquire new scientific and technical knowledge and a platform to influence U.S. public opinion, while U.S. visitors to China are selectively chosen by the Chinese authorities and given little more than "guided tours" (p. 49). Diplomatic recognition, as Barnett acknowledges, will not automatically change the substance of relationships; his issues are presented as an agenda to be worked on over the next 10 to 30 years. This reviewer feels that the drastic action of derecognition of Taiwan which Barnett proposes is scarcely worth the opportunity to work out his agenda of issues over the next three decades. In addition, U.S. derecognition of the Republic of China may lead the Taiwan government to seek allies elsewhere—rumors of "feelers" between it and the Soviet Union have been widely circulated—thus seriously destabilizing the East Asian equilibrium rather than, as Barnett hopes for his plan, further stabilizing it. I agree wholeheartedly with Barnett's contention (p. 118) that Peking's positions are often "more complicated than its general posture implies, and it has demonstrated that when China's own economic interests demand flexibility it can modify its views." Perhaps rather than take the precipitous action Barnett proposes, U.S. policy makers would do well to keep this in mind.

JUNE TEUFEL DREYER

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International Arms Control: Issues and Agreements. Edited by John H. Barton and Lawrence D. Weiler. (Stanford: Stanford University Press, 1976. Pp. ix + 444. $18.50, cloth; $12.95, paper.)

A colleague once asked Albert Einstein how it was that man could unlock the physical secrets of the atom and still be unable to devise political means to keep the atom from destroying civilization. "That is simple," Einstein replied. "Politics is more difficult than physics." If Einstein was correct, then arms control is more difficult than either physics or politics. Understanding arms control requires a knowledge of diplomatic negotiations, science policy, and bureaucratic in-fighting, as well as some comprehension of physics and politics.

Arms control is a complex and diversified enterprise. Perhaps that is why most writers in the field have chosen to specialize on some more narrowly defined topic: international arms trade, nuclear proliferation, or U.S.-Soviet strategic arms limitation. There has long been a need for an integrative work, a volume which would weave together the various issues which constitute arms control.

International Arms Control fills this need commendably. It analyzes the history of arms control, its cultural context, its institutional setting, the impact of economics, and all of the postwar efforts—successful and unsuccessful—to conclude arms agreements. Not since Hedley Bull's 1961 work, *The Control of the Arms Race*, has there been such a useful guide to the intricate and often perplexing problems of limiting arms and armies.

The book is a collective effort by the Stanford Arms Control Group, an assemblage of political scientists, physicists, lawyers, historians, and systems analysts. The group jointly taught a very popular undergraduate arms control course at Stanford, and the text is derived largely from their lectures. This is probably one of the best books ever written by a committee. In fact, the book's success is largely the synergistic consequence of bringing together representatives of so many different disciplines.

Most arms control literature is either political—that is, gives primary attention to political problem—or technical in nature. Not enough work in the field attempts to relate the political and technical aspects of arms control. (A large share of the exceptions to this rule can be found in two journals, *International Security* and *Scientific American*.) The most attractive feature of *International Arms Control* is its authors' skill in combining politics and technology. That fact alone makes it a very useful text.

Anyone who has ever taught a course in arms control or defense strategy has probably found the class divided into two distinct groups. On the one hand are political moralists who desperately want to rid the world of war; on the other are the technologists, or to put it simply, the hardware freaks. Technologists are apolitical. They are much happier discussing the intricacies of, say, stellar inertial navigation than the morality of mutual assured destruction. Neither group is eager to engage in the other's favorite subjects, and it is a major pedagogical challenge to find some common ground for classroom discussion.

Each group has something to learn. The moralists must appreciate that value judgments cannot be made without some understanding of military innovation; technologists must know that new weapons are not simply laboratory playthings, but may have a major impact on the likelihood of war.

International Arms Control is an excellent vehicle for structuring a meaningful dialogue. The book covers important technical considerations in a jargon-free prose comprehensible to laymen. It also analyzes the significant political dimensions of arms control, relating them to new weapons developments in a manner which even the technologists may find appealing. Two examples will illustrate the point.

Most political moralists take it as a matter of faith that nuclear bombs are weapons of unthinkable destructive power. Yet not one in a thousand could describe how a nuclear weapon works or what it does. In a few pages *International Arms Control* explains the difference between an atomic and a hydrogen bomb, the prompt effects of a nuclear detonation, and the likely results of a large-scale nuclear attack. Political moralists are right: nuclear weapons are enormously destructive. And they ought to know why.

To most technologists, ABMs and MIRVs are engineering marvels, far more interesting as pieces of hardware than as problems of defense strategy. The authors of *International Arms Control* wisely chose to consider these two weapons systems in a chapter devoted to strategic doctrine. The text explains not only how ABMs and MIRVs work, but also how they affect nuclear strategy.

For all of its virtues *International Arms Control* does have a number of minor deficiencies. Several important issues are given inadequate attention. One might have hoped for a more thorough analysis of verification procedures and requirements, the Outer Space Treaty, and chemical/biological arms control, to name just three. The discussion of nuclear proliferation is unforgiveably weak. One brief chapter (20 pages) is devoted to what may well be the most important arms control problem of the next decade, yet most of that chapter is given over to a rather dry recital of how the Non-Proliferation Treaty was negotiated.

Perhaps the most serious criticism of *International Arms Control* is that it takes too adulatory a view of arms control. In theory everyone likes the restraint and caution which the term arms control suggests, but in practice the virtues and vices of arms control are hotly debated. They should be. There are risks involved in arms control, and many critics have argued seriously that agreements may serve to raise arms levels rather than reduce them. Aside from a brief treatment in the concluding pages, the authors of *International Arms Control* ignore the many serious and legitimate questions raised about the utility of arms control.

This may be unfair criticism. After all, one doesn't fault the Bible for failing to give Satan an opportunity for rebuttal. But arms control is not theology, despite the attitude of its more ardent supporters. Arms control true believers have much to learn from heretics and atheists.

Some critics will charge that this work covers little new ground. That is true, but irrelevant. The book's purpose is not to develop new ideas, but to integrate existing ones—to pull together into one volume an assessment of the many forces which shape arms control and defense policy in the contemporary world. On that score it succeeds admirably. For a college course on the subject, *International Arms Control* is the best single textbook on the market today.

JOSEPH KRUZEL

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The Paralysis of International Institutions and the Remedies: A Study of Self-Determination, Concord Among the Major Powers, and Political Arbitration. By Istvan Bibo. (New York: The Halsted Press, Wiley, 1976. Pp. xi + 152. $17.50.)

Istvan Bibo, a Hungarian intellectual who served as a Minister of State during the final days of the 1956 uprising, has set out in search of an answer to the problem of peaceful settlement in an age when moribund international institutions have proved incapable of dealing with disputes relating to the formation of states and the territorial integrity of these units. While the Cyprus question and the Arab-Israeli conflict motivated the author to undertake this quest, he refrains from expounding on topical political questions and makes only sparing reference to these and other specific examples in the course of his analysis. Approaching the problem of conflict resolution from a broad, historical perspective, Bibo embarks upon an informative and insightful examination of the legal principles that form the basis of the contemporary international system. This inquiry culminates in a well-reasoned argument for the universal adoption of the right of self-determination as a new basic principle of international order. For Bibo believes what is required to remedy the current paralysis of international institutions is both the better functioning of existing agencies of great power agreement and the emergence of new international institutional arrangements for impartial political arbitration based on democratic principles.

At the root of the present problem of conflict resolution, according to the author, is considerable confusion over the fundamental principles which could serve as generally valid premises for the practical application of peaceful settlement procedures. In short, existing mechanisms for resolving international disputes lack legitimacy. Within any social community, Bibo feels, "The ultimate basis for a claim to legitimate power is a common conviction that those exercising power are in fact competent to do so" (p. 10). This common conviction usually takes the form of certain governing principles. Looking back nostalgically at history, he finds that the governing principles of monarchic-feudal legitimacy provided the basis for an orderly and generally peaceful system of conflict resolution in Europe. However, once the divine right of kings gave way to ideas of popular sovereignty and the old order collapsed, the principles associated with monarchic-feudal legitimacy were never entirely repudiated, nor were new principles expressing the sovereign will of the people ever fully accepted. The tragic result is that existing laws and mechanisms dealing with "state-formation" and territorial settlement are more or less a conglomeration of unworkable old practices and embryonic procedures. Thus, the world of today needs to come to some basic agreement on a new set of governing principles of it hopes to recapture the order and stability of the past.

In Bibo's mind, the right of self-determination should serve as the major legitimizing principle underlying this new international order. Although references to the term "national self-determination" are studiously avoided, he does see a definite relationship between the concept of nationhood and the right of self-determination. A nation is a social community whose aim, in the author's words, "is political in that it aspires to well-defined territory, organised so that the national consciousness is shared by most if not all of the community" (p. 35). The right of self-determination asserts that the community is founded upon the democratic principles of liberty and equal human dignity, and that the aspirations and convictions of the community reflect those of the majority. Moreover, the principle of self-determination implies a willingness not only to tolerate diversity at home but also to accept pluralism in the world at large.

While Bibo rejects the idea of world government as impractical, he does not consider the nation to be an absolute or everlasting phenomenon. Nor does he believe that the principle of self-determination will stand as an impediment to international integration. Rather, the application of the principle of self-determination will insure that supranational organizations are founded upon democratic principles and thus reflect certain fundamental human rights and aspirations. In the interim, the right of self-determination will provide the keystone for the creation of a stable and legitimate order in which all nations can seek to resolve their disputes peacefully.

At the conclusion of his essay, Bibo deals with the related problems of getting the principle of self-determination accepted and creating institutions and procedures for putting it into practice. The great powers hold the key to the principle's eventual acceptance, for they must be willing to forego blind political considerations and reach a common understanding that recognizes the right of self-determination if this new order is to emerge. Once this agreement is reached, the principle of self-determination could be given practical application through an impartial international tribunal of political arbitration, the organization and procedures of which are spelled out in detail by the author.

In citing the need for great-power concord, Bibo is cognizant of the fact that law is not totally divorced from politics. Nevertheless, he tends to view the problems of "state-formation" and territorial settlement from a purely legal perspective. More importantly, with renewed emphasis being placed on human rights, one is led to ask whether the basic principle of self-determination should not be first applied by the states themselves in hopes of achieving a stable and legitimate internal order before tackling the international community.

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Civilization in Crisis: Human Prospects in a Changing World. By Joseph A. Camilleri. (Cambridge, England: Cambridge University Press, 1976. Pp. viii + 303. $17.95, cloth; $5.95, paper.)

Joseph A. Camilleri concludes his splendid search for a new world order with a call for "affirmation of man's triumph over death" (p. 261). Where it is understood as rage against what Rollo May has called "the prototype of all injustice" (*The Courage to Create*, p. 25), this call sums up a book that combines a convincing indictment of the present human condition with a compelling plea for creative planetary renewal. Bent upon coping with myriad elements of the *problématique humaine*, Camilleri answers the endless scream of our nightmarish world with an imaginative and purposive reformist strategy. The product of this response is a richly rewarding work, one that confronts the individual human roots of our apocalyptic predicament with a subtle prescription of Dionysian vitality and Apollonian order.

Civilization in Crisis is, above all else, a "world order" book. Resting openly on normative criteria, it embraces an auspicious combination of systemic, holistic, futuristic, and multidisciplinary orientations in seeking a more harmonious configuration of global life. Utopian in its vision, it is nonetheless aptly sensitive to problems of implementation and modalities of transformation. The book, therefore, is firmly in the tradition of global improvement strategies which ranges from the recommendations of Dante and Pierre Dubois in the fourteenth century to the "preferred worlds" of Richard Falk and Saul Mendlovitz in our own time. A major effort in this tradition, it ties the avoidance of Armageddon to a proper union of thought and action.

Focusing attention on the decadence of our current industrial culture, the absurdity of our economic and political institutions, and the dangers of ecological and military spoliation, the author progresses from an overview of today's world crisis to the articulation of a new politics for survival. Symptomatic of this crisis is a widespread condition of "psychological disorder" that calls into question the most elementary forms of social organization. Faced with worldwide instances of economic exploitation, human dislocation, and resource depletion, Camilleri describes a planetary habitat that is a grotesque distortion of humankind's enormous potential for improvement, a hideous parody of what might have been. Teeming with "gigantic war machines" that produce only growing levels of insecurity, it is a habitat with an ever-widening discrepancy between technical intelligence and reason.

To reverse this portentous state of affairs, *Civilization in Crisis* is dedicated to the construction of a reliable ark of human renewal. A new human community, we are told, must arise to counteract the fundamental disequilibrium which now vitiates humankind's capacity for biological and cultural adaptation. Such a community, based upon a reorganization of global economic patterns and the development of a universal culture, would be implemented consensually, balancing the need for regulation with the imperatives of autonomy and diversity. Within its boundaries, which would necessarily be coextensive with the planet as a whole, survival might once again be taken for granted, not only in the obvious physical sense of existence, but in the spiritual sense of dignity or *humanitas* as well. Camilleri's hope for a true praxis of liberation envisions not only a halt in the disintegration of human life-support systems, but the systemwide acceptance of a new life-affirming ethos. It would, in short, replace T. S. Eliot's "hollow men" with men and women who truly fulfill their own being in the world.

The only noteworthy shortcomings of *Civilization in Crisis* concern the author's misconceived extension of certain nonviolent principles from domestic to international affairs. Camilleri's advocacy of the concept of "civilian defense" whereby "a resistance movement would make it virtually impossible for the enemy to establish and maintain political control in spite of vastly superior military power" (p. 258) betrays a fundamental misunderstanding of today's global "balance of terror." What we must fear is not, as Camilleri alleges, the tyranny of military occupation, but nuclear attack from distant bases. Here, the idea of "civilian defence" is completely out of place.

But these minor shortcomings are overshadowed by what is otherwise a sound and fruitful inquiry. *Civilization in Crisis* deserves a wide audience among all students, scholars, and informed general readers with an interest in the human prospect. Its exceptional range of insight and erudition will reward every reader who shares the author's timely hope for a tolerable world future.

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International Politics of Energy Interdependence: The Case of Petroleum. By Nazli Choucri with Vincent Ferraro. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xv + 251. $20.00.)

The tremors of the fivefold increase in the global price of oil that was imposed by OPEC in 1973-74 have released an avalanche of reports, forecasts, proposals, monographs, and studies. Among these, Nazli Choucri's book has the merit of combining relevant economic information with an analysis that "emphasizes political elements" (p. xiii). Unfortunately, the economic and political data remain inadequate at crucial points, and the conclusions unconvincing.

The book reviews the structure of the international petroleum market as developed by the seven major companies in the decades before 1973, and the rise of OPEC in the 1960s and early 1970s; the financial impact of the price jump on the global economy (what has become known as the problem of "petrodollar recycling"); the links between the oil crisis and Middle Eastern politics; and the attempted countermeasures of the importing countries.

There are many perceptive points in the analysis. OPEC members are classified into four groups (p. 40 ff.): those interested in higher revenues that have relatively high reserves (Iraq, Iran) or who lack such reserves (Algeria, Venezuela, and others), and those with high reserves and low revenue needs that have pursued more conservative policies (Saudi Arabia, United Arab Emirates), or have taken a more radical stance (Kuwait, Libya). The threat of invation as a countermeasure is judged militarily feasible but politically prohibitive (p. 163).

I am inclined to quarrel with other points. "The first price increases by the oil-exporting states" came not in 1973, as stated on p. 53, but in 1970-71, as is acknowledged on p. 39–although even there the importance of the Tehran and Tripoli agreements as the crucial turning point in company-OPEC relations is underrated. Among the "de facto members of OPEC" (p. 43)–who have followed its price increases without joining the organization–Choucri mentions only Norway, China, and Canada. Yet at the time of her writing, the largest non-OPEC exporters at OPEC or near-OPEC prices were the Soviet Union, Oman, Trinidad, and Malaysia.

More serious is the author's failure to consider the central role of Saudi Arabia, whose excess oil production capacity is larger than the total production of any other OPEC member except Iran. Hence the Saudis, by increasing production, can prevent any price increases of which they disapprove–this was the lesson of the dual price structure within OPEC early in 1977 but fully demonstrable beforehand. They also can discourage any all-out price war in which they alone would be the ultimate winners (cf. D. A. Rustow and J. F. Mugno, *OPEC: Success and Prospects*, New York: N.Y.U.

Press, 1976, p. 99 ff.). Hence OPEC's price cohesion depends less on the potential political tensions among its 13 members which the book so elaborately (and somewhat inconclusively) discusses than on the stark economic reality of the Saudis' surplus capacity.

In assessing the future, the author rightly predicts that "new sources of petroleum will be available ... and new sources of energy will become commercially viable" (p. 182), and concludes that these "eventually" will lead to OPEC's breakup. She fails to ask at what *price* non-OPEC oil and non-oil energy will be sold. Experience since 1973 indicates that North Sea oil, U.S. coal, uranium and other energy sources have followed OPEC's price lead, rather than providing a competitive ceiling.

These are crucial gaps for the book's central argument. "The important question," Choucri writes (p. 47), "is not whether OPEC will disintegrate but *when* and *how*." Of course, as Keynes has said, in the long run we shall all be dead, including Sheikh Yamani, the Shah, Colonel Qaddafi, and their successors. But within the time frame implied by this book, the author simply does not demonstrate that OPEC *will* break up somehow or other.

Her specific conclusion that "another Middle East war would almost inevitably destroy OPEC" (p. 183) is downright preposterous. True, Iran and the "non-Middle Eastern producers will not participate in any embargo"—nor did they in 1973. But meanwhile U.S. imports of crude petroleum and petroleum products in the first half of 1977 were 40 percent above 1973 levels, so that they now constitute the equivalent of 75 percent rather than 42 percent of non-Arab OPEC production. Since it was the 1973 embargo that launched OPEC on its meteoric rise, why would a repetition of the experiment under vastly more favorable circumstances "almost inevitably destroy" it?

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Comparative Patterns of Foreign Policy and Trade: The Communist Balkans in International Politics. By Cal Clark and Robert L. Farlow. (Bloomington, Ind.: International Development Research Center, Studies in East European and Soviet Planning, Development, and Trade, No. 23, 1976. Pp. xii + 152. $6.00, paper.)

By now, it is a commonplace that foreign policies of East European states, especially the Balkan ones, diverge significantly from each other and from that of the USSR. While some studies have been made of Albanian, Romanian, and Yugoslav foreign policies, little comparative analysis has been done. This monograph represents a modest but useful beginning. Its aim is to outline and analyze comparatively the foreign policies of Albania, Bulgaria, Romania, and Yugoslavia, to identify the domestic factors behind their different foreign policies, and—this is the original contribution of the study—to examine the linkages between foreign policy and foreign trade.

Although the authors analyze four concrete cases, they choose to call them "models," by which they mean "theoretical and simplified representation(s) of the real world" (p. 5). However, instead of "theoretical representations" we have here abbreviated, but balanced and penetrating, analyses of four types of foreign policy patterns: "nonalignment" (Yugoslavia), "partial alignment" (Romania), "realignment" (Albania), and "subalignment" (Bulgaria). Clark and Farlow trace the development of the foreign policy and foreign trade policy of each country in order to answer their *main analytical question: do changes in foreign* trade patterns "cause" foreign policy changes, or are they a "result" of such changes? In other words, does trade follow the flag, or vice versa? Their general conclusion is that "trade was a much better result than causal indicator" (p. 101), though Romania is a partial exception. They recognize that patterns of foreign trade are determined not only by political factors, but by such economic ones as complementarity of economies, internal economics structures, and the kinds of commodity composition associated with different trading partners. Thus, we observe several instances where what was politically desirable for a Balkan state was economically unfeasible, and so trade cannot serve as a reliable indicator of political relations.

Differences in Balkan foreign policies are explained largely in terms of elite perceptions of personal and national interests, in different historical, cultural, and situational contexts. These are standard descriptions of the foreign policies of the Balkan Communist states, but the analyses of foreign trade policies are more original, and equally persuasive. In the case of Yugoslavia, trade turns out to have been a good indicator of its political relations before 1965, but after the economic reform of that year trade appears to respond more directly to economic forces. In Romania, economic and political motives reinforced each other from 1958 to 1968, and pushed Romania toward the West. Since that time, economic problems "generated in this process, began to limit the Romanians' Westward tilt and began to hinder trade's ability to fully reflect political developments" (p. 64). Albania's economic requirements have also forced it to trade more with Eastern and Western Europe than one might guess from its Stalinist-Maoist rhetoric. Bulgaria, too, has had to compromise political positions in increasing trade with the West, on one hand, and inhibiting it with Third World countries, on the other.

Perhaps the one major factor absent from the analysis is the influence of changing domestic economic goals and priorities on foreign trade policy. Though the relationship is clearer in the countries of the "Northern Tier" of Eastern Europe, the last decade has seen much of foreign trade policy determined by growing consumer pressures and a reordering of domestic priorities. Clark and Farlow ignore this.

Aside from minor lapses—for example, Romania is said to have established diplomatic relations with Israel in 1969 (p. 63) when, in fact, it never severed them—the monograph is carefully and logically organized, well documented and enhanced by statistical appendices. It treats the foreign policy-foreign trade relationship in a subtle and sophisticated way, and points the way to further research on an issue of great importance not only to Communist area specialists but to those engaged in the comparative study of politics and of foreign policy.

ZVI GITELMAN

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Public Papers of the Secretaries-General of the United Nations, Volume 6: U Thant 1961-1964. Edited by Andrew W. Cordier and Max Harrelson. (New York and London: Columbia University Press, 1976. Pp. 708. $27.50.)

One of the more formidable consequences of the conjunction of latter-day printing technology and the compulsion, often borne of the desire to survive academically, is the veritable deluge of document anthologies. Such weighty tomes even now threaten to exhaust library space, absorb limited foundation money, and stifle human thought. As with notable—and, let us hope, deviant—examples in the numbers revolution, massive and insignificant doodles are detailed and catalogued for what appears to be no other reason than that they are there. The temptation to array such scribblings before the literate public is especially keenly felt when the author of such papers is or occupies a position of at least putative importance. The reader could therefore be excused if he passed by this latest volume in the collected papers of the Secretaries-General of the United Nations in the belief that such collections of tedium should be relegated to well-deserved obscurity. Although such instincts are often well justified, in this particular case such resistance should perhaps be qualified.

Whatever the changeable fortunes of the United Nations Organization, the role of the Secretary-General has been more extensive, visible, and controversial than was envisaged by the drafters of the Chater. Although with at best problematical effect, the occupant of the office from Trygve Lie to Kurt Waldheim has been intimately involved in a remarkable number of the issues which have shaken the world since World War II. And U Thant, as leaseholder of the office from 1961 to 1971, was certainly on the scene in a period of tumult ranging from the Congo Operation and the Cuban missile crisis to the Six-Day War and Vietnam. Although the skeptical reader will find banality in abundance in this volume, the editors may nonetheless be forgiven the attempt to order the utterances of the former Secretary-General—and the occasional insights into the mind of U Thant probably justify the effort.

The papers in this volume covering U Thant's stewardship from 1961 to 1964 make abundantly clear that he held a notion of the office of Secretary-General comparable to that of Dag Hammarskjöld and Trygve Lie—not simply constrained by the mandates of the General Assembly and the Security Council but possessed of independent powers of initiative. Moreover, he enunciated the "two Thant" doctrine whereby he permitted himself to make public statements either in his official role or as a private individual—not, as it might be imagined, a particularly neat bifurcation. At the same time, he conceived the Secretary-General as representing not only collective/state interests but as a repository and spokesman for the moral conscience and universal aspirations of mankind—a kind of secular papacy. And, however much he insisted on the critical conjunction of material power and interest, the papers give the impression that he particularly enjoyed his stewardship over the moral imperium.

In that role, he saw the UN and the Secretary-General as primary in the formulation of the agenda of the universal community of mankind. Colonialism, racism, inequality, and the very survival of the race defined, in his view, the pressing problems of the age whereas he appears to have perceived the cold war as a dangerous delusion. To the degree that substantial ideological issues are involved in the contest between the West and the East, he held that they would be overcome in practice by convergence in the political systems of the principal antagonists. He thus believed these issues to be increasingly irrelevant to the real aspirations and needs of mankind or a diversion from more critical problems and a threat to global peace. In perusing his papers, one finds it at least understandable that many in the West and East at the time saw his moral judgments as presumptuously removing from the human agenda political and ethical questions central to their concerns.

Despite U Thant's at times immodest view of his office, the record nonetheless demonstrates that he spent a good deal of his time mending the fences between the Secretary-General, on the one hand, and the USSR and France, on the other, which had fallen into such disrepair in the latter months of Hammarskjöld's tenure. At the same time, he attempted to square his initiatives as closely as possible to the mandates of the General Assembly and the Security Council. This is especially evident in the liquidation of the Congo Operation—an enterprise which above all others had incurred the enmity of France and the Soviet Union and had fundamentally upset the finances of the organization. Even in the Cyprus Operation, which had bestowed on the Secretary-General unconscionable responsibilities for arranging the forces and their financing as well as mediatory tasks, U Thant sought to maintain at least non-hostile passivity on the part of France and Russia.

In terms of major initiatives of U Thant in the first period, Cordier and Harrelson gathered together papers on the 1961 bond issue, the Goa dispute, the West Irian problem, the Thailand-Cambodia border controversy, the Yemen civil war, and the dispute among Malaya, Indonesia, and the Philippines over the states of North Borneo (Sabah) and Sarawak. And, of course, there are the statements and exchange of messages during the Cuban missile crisis. Curiously, it was the latter intervention which gave U Thant the most satisfaction, whereas there is little either in this record or elsewhere which demonstrates the centrality of the Secretary-General to the resolution of the crisis unless one sees, as some do, his proposal for a temporary Soviet suspension of arms shipments to Cuba and a corresponding U.S. suspension of the proposed quarantine measures as providing the critical formula for the resolution of the conflict. And perhaps it is in the interstices of conflict, in the provision of good offices, that the importance of the office of the Secretary-General lies, rather than in daring initiatives or moral pronouncements. In any case, this volume provides useful insights into the mind and practice of one occupant of that office—with all the contradictions, ambiguities, and defects in moral and political vision, as well as patent strengths and virtues, involved. As in any such anthology, one can note that much in the collection is of less than heroic interest, but the canons of public papers editorship seem to demand erring on the side of superabundance than selection. Presumably, the latter is left to the biographer who is to benefit from these formidable tomes.

ROBERT S. WOOD

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Foreign Policy Motivation: A General Theory and a Case Study. By Richard W. Cottam. (Pittsburgh, Pa.: University of Pittsburgh Press, 1977. Pp. vii + 374. $12.50.)

The most important—but admittedly elusive—requirement for foreign policy formulation, according to the author of this book, is an accurate understanding of the motivations of other governments and peoples. Richard Cottam's normative purpose is "to deal with the perceptual base of conflict," and his analytical purpose is to devise means for inferring motivational attributions "largely from policy preferential statements." As suggested by the subtitle of the book itself, this task is pursued by way of "a general theory and a case study"—the latter involving the British in late nineteenth-century Egypt. In contrast to the "motivational monocausality" that characterizes the basic assumptions of much traditional (and current) theory, the author defines motivation in terms of "a compound of factors that predispose a government and people to move in a decisional direction in foreign affairs."

Having devoted early pages of the book to an elaboration of his own assumptions, Cottam presents brief but telling critiques of a number of widely recognized approaches to foreign policy motivation. Hans Morgenthau and the realist school clearly point "to a simple power determinism in which behavior flows inexorably from the relative power of potential actors," Cottam writes, on the oversimplified assumption that "collective man, like individual man, is power driven." According to William Riker, by contrast, what the rational political man really wants "is to win, a much more specific and specifiable motive than the drive for power." More precisely he seeks "the smallest possible winning coalition . . . but never knows quite what it is"—with the result that he pays too much for his alliances and thus weakens himself into eventual defeat. And so the monocausal explanations go. "Even Thomas Schelling, who rebels against the zero-sum quality of

Riker's work and asserts instead mixed motives, really theorizes in terms of an enemy who seeks to 'win' and must be deterred from doing so."

Unfortunately, according to Cottam, the "most uncompromising opponents of U.S. strategic doctrine and cold war practice, the revisionist historians," operate from a base—economic determinism—that is "hardly less simple or determinist" than the power determinism of the realists and deterrence specialists. Thus, from the theoretical assumptions on both sides, it follows logically that each side in the cold war debate has thought of the other side as "imperialistic."

Anyone who has worked conscientiously in the sphere of international relations and policy making must surely have sensed the tension there between parsimony (highly valued in scientific endeavor) and explanatory sufficiency. No doubt, much of the "compelling quality of the realist argument" which Cottam freely recognizes and which is "testified to by its widespread acceptance by practitioners of diplomacy and by probably most writers in the field of international politics" derives from its very simplicity and ready applicability. Much the same sort of thing can be said for the wide acceptance, by others, of economic determinism. Both approaches avoid an enormous amount of uncertainty, indecision, and galling frustration. But with respect to parsimony and explanatory sufficiency, we seemingly cannot have it both ways. Consequently, for the scholar, at least, something along the lines of Cottam's plunge into complexity very nearly qualifies as an imperative.

Cottam sees fewer difficulties in dealing with system values for a collective than for an individual. In regard to a state, he proposes attention to three "choice areas" as adequate: the priority placed upon peaceful settlement of disputes; the level sought in relative influence in world affairs; and the actor system called for in world affairs. The dynamics of change as he sees them are essentially those of balance: As between two countries, consistent nonrealization of expectations of specific policy decisions "will lead first to almost imperceptible policy shifts until the two powers have a *de facto* changed relationship. Then at some point verbalization of the changed relationship may be brought up to date with the *de facto* change."

The author slides over—perhaps too readily—the relevance to his approach of Abraham Maslow's hierarchy of needs, Richard Snyder's treatment of individual motivations, and the use of aggregations of demographic, economic and military statistics. An examination of goal statements and policy rationalizations reveals, however, that national leaders are often extremely sensitive to domestic demands that can be categorized rather neatly in terms of Maslovian "need" levels and that have strong implications for foreign policy. Official rationalizations of the Marshall Plan, Point IV, and recent demands by leaders of Third World countries for a restructuring of the international trade and monetary structures are only a few of the many examples that could be cited.

As for the influence of personal motivations in the design and implementation of foreign policy, there is voluminous material from analyses of Wilsonian policy, Kennedy's behavior during the 1962 Cuba crisis, and so on: whatever the broader, societal considerations may be, all are, to one degree or another, "filtered through" the personalities of responsible national leaders and are therefore subject to the influence of personal perceptions, evaluations, expectations, and other cognitions and feelings of individual human beings.

When used as a source for inferring motivations, aggregate data are undoubtedly subject to abuse, as Cottam rightly points out, but neither can they—nor the phenomena they represent—be wholly ignored. It is probably true that conquest rarely results directly from "a desire to relieve population pressure," but substantial growth (or decline) in the numbers of people as well as in their technology and economy can upset a prevailing "balance" in capabilities as well as domestic demands for resources, markets, investment, protection of trade routes, balance of payments, attitudes toward international trade and monetary structures, and so forth.

Admittedly, such considerations may be exceedingly difficult to incorporate within a given theory or a particular model, and even more difficult to treat methodologically, but that does not mean that we are relieved of the problem of what to do with them—especially within "a general theory" of foreign policy motivation. Similarly, it appears increasingly evident that neither motivation nor behavior—perhaps especially with respect to foreign affairs—can be dealt with adequately unless a great deal of attention is paid to the implications of complicated feedbacks, which require, in turn, a theoretical base that is at least minimally capable of encompassing intensely interactive and dynamic relationships from a variety of sectors.

This is a very large order, of course, and is not likely to be achieved by any single study for a long time to come. Yet it appears to me that several, if not all, of these considerations could be encompassed to good effect in the underpinnings, if not in the main body of Cottam's framework. Meanwhile, Cottam has successfully challenged some grossly oversimplified and potentially misleading assumptions that have been widely accepted and frequently acted upon virtually up to the present. Beyond this, he has also dealt successfully with several of the pernicious problems that must be confronted in the pursuit of a more developed general theory.

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Latin American Foreign Policies: An Analysis. By Harold Eugene Davis, Larman C. Wilson and others. (Baltimore: The Johns Hopkins University Press, 1975. Pp. xiii + 470. $18.00, cloth; $5.95, paper.)

It is legitimate to speculate that in social scientists' strenuous search for abstractions and theoretical material—more often than not restricted to trivia—the burden of dealing with the substantive problems of Latin America is gradually going by default to those, outside and inside academia, with firm ideological commitments, generally of the political left, who are willing to undertake the arduous task of collecting data, describing events, and generally grappling with the meaty material of Latin America's problems. Their work may reflect their political biases but it fills a vacuum, and is therefore useful. Davis' and Wilson's scholarly book, free from political predilections, does not fall in this category, but it effectively accomplishes the tasks just outlined.

This volume is a collection of 20 essays on the historical antecedents, the formulation of the foreign policies, and the international behavior of the Latin American nations. Edited by an experienced Latin Americanist in collaboration with a younger scholar, this anthology has the merit and novelty of being the first book of its kind to approach the subject from the perspective of each of the individual states in the region. Fifteen of the essays examine the foreign policy of one nation or a small cluster of states (as in the cases of the Central American and the Caribbean republics); the other five deal with general background aspects on a regional basis or with some specific topics such as problems of economic relations, the OAS, or Vatican diplomacy.

The opening chapter, written by Harold Davis, aptly sets forth the purpose of the editors, defines the problems, and identifies the lines of analysis which the contributors were asked to follow. Briefly, these are: an examination of the major international problems confronted by the nation in question and the historical antecedents of these problems; an explanation of how its foreign policy is formulated; and a characterization of the self-image of the nation on the international scene. The editors rightly deplore the tendency of earlier works to view the region as a unit, which has so often distorted U.S. views of international affairs, and persuasively point out the merits of their chosen country-by-country approach.

Among the 16 contributors, selected as country specialists, there are historians, economists, and political scientists who provide a useful variety of disciplinary interests and perspectives, and also represent two different generations of scholars.

Despite the effort of Davis and Wilson to provide their collaborators with a tightly drawn design for the analysis and although a reasonably high level of scholarship is maintained throughout the volume, the essays vary considerably in emphasis and approach, exhibiting the chronic unevenness and the abrupt shifts of perspective and style peculiar to works of this type. It is to their credit that the editors acknowledge the inevitability of this weakness and plead for understanding in their introduction.

Undoubtedly, some will be disappointed at the meagerness of true analysis and the relative absence of elaborate and sophisticated "theoretical frameworks" and "conceptualizations" and "theses" so often obsessively sought at the expense of substance. These critics should refer to the editors' statement of purpose. They make it clear that this is a textbook, intended to serve the needs of advanced undergraduates and beginning graduate students in political science, history, international economics, and Latin American studies. As such it is essentially descriptive and not doctrinaire and speculative, and represents a job that very much needed doing. It may not lead to the higher altitudes of theorizing but it is informative and helpful.

The book has some weaknesses. The role of multinational corporations and the emergence of regional bargaining units are given little attention. It does not examine the changing relationships between Latin America and the United States and the Third World. It does not deal with the contest for leadership in the region's international affairs nor with the emergence of new and ambitious actors such as Venezuela and Cuba. But, still, it is a commendable work of value to students of Latin America.

FEDERICO G. GIL

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Managing an Alliance: The Politics of U.S.-Japanese Relations. By I. M. Destler, Priscilla Clapp, Hideo Sato, and Haruhiro Fukui. (Washington, D.C.: The Brookings Institution, 1976. Pp. viii + 209. $9.95, cloth; $3.95, paper.)

Among the increasing number of books and articles on U.S.-Japanese postwar relations, this book represents one of the more remarkable attempts to clarify and give solutions to the problems of "managing an alliance" between the world's first- and third-ranking economic powers. The strength of this study lies in the well-balanced combination of historical perspective, which sheds light on the events that took place, and a political science perspective, with an explicit analytical framework, which focuses upon how and why things happened the way they actually did.

To illustrate "the interplay between domestic politics and U.S.-Japanese relations" (p. 8), the authors examine three postwar episodes: revision of the bilateral security treaty in 1960; agreement on reversion of Okinawa to Japan in 1969; and the dispute in 1969-71 over Japanese textile exports to the United States. Throughout the book, the Okinawa episode and the textile episode are contrasted in order to show what caused different results, i.e., a success in the former and a failure in the latter, in "managing an alliance." The authors employ the approach of bureaucratic politics as their analytical framework, based upon their view that "the major foreign policy actions ... have their roots in politics at home, both bureaucratic politics within each government and the broader national interplay labeled domestic politics" (p. 3).

Chapter 2 examines the postwar setting of the bilateral relations and each of these three cases through the historian's eyes. It amply demonstrates the importance of the historical perspective for the analysis of foreign policy making. Foreign policy making in Japan and the United States is comparatively analyzed in chapter 3 with heavier emphasis on the Japanese side. Japan's political system is examined in terms of its structural components (LDP, bureaucracy, the big business community, opposition parties, labor unions, academia, intellectuals, and the press) and the relations among them. The overall pattern of Japanese politics is characterized as "sharp differences over the fundamental lines of foreign policy" (p. 59) between the conservative establishment and opposition groups.

The most fascinating part of the book is chapter 4, where the authors attempt to analyze the recurrent pattern of misperception which poses "continuing problems" in U.S.-Japanese relations. Misperceptions are categorized into two types: those "rooted in politics as usual" and those "rooted in cultural differences." Their analytical framework, the bureaucratic politics approach, is very skillfully used to analyze the first type which arises "inevitably in relationships between any two major democratic countries with complex and differing political and institutional structures" (p. 90). The authors then argue that the second type is "peculiar to the U.S.-Japanese relationship" (p. 90). One may wonder why it is peculiar to this particular bilateral relationship, because any bilateral relationship necessarily involves certain "misperceptions rooted in cultural differences." It can be found in relations between allies (e.g., Anglo-American relations) as well as between adversaries (e.g., U.S.-Soviet relations). The difference seems to be one of degree rather than of substance. The problem in the case of the textile issue, according to the authors, lies in the failure of both policy makers to "give much weight in their political calculations to the demands of politics on the other side of the Pacific" (p. 124). This kind of problem seems to occur rather frequently in many bilateral relations. The problem can be serious, as in the textile case, depending upon the degree of political as well as cultural differences between the countries involved.

Chapter 5 widens the scope of analysis to examine the interplay of the two national systems focusing on the initiation of issues, the politics of interaction, and the resolution of issues. The most important variable here is communication between the two foreign policy-making systems. The Okinawa case represents a successful communication in terms of its timing and combination of channels. The textile case was a failure in this respect (p. 165). In assessing causes of success and failure in this bilateral relation, examination of differences in the nature of issues seems to be given a somewhat perfunctory glance. A more detailed and systematic examination of this factor would give us better insight into the causes of both policy makers' failure to be more sensitive to their counterpart's positions in their respective domestic politics in the textile case.

In conclusion, the authors suggest that the policy makers in each country, "be sensitive to the position of their counterparts in the other, and try to shape their official actions so as to strengthen these counterparts" (p. 195) in order to "manage" this bilateral alliance better.

In spite of a few dissatisfactions, I have no doubt that this book is one of the most important works in this field and no one who is concerned about "managing an alliance" between the two most powerful economic giants in the world can afford to ignore it.

TSUTOMU ODANI

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The European Community in World Affairs: Economic Power and Political Influence. By Werner J. Feld. (Port Washington, N.Y.: Alfred, 1976. Pp. xiii + 352. $9.95, cloth; $6.50, paper.)

Werner J. Feld, author of three major works on the European Community, *The Court of the European Communities: New Dimension in International Adjudication, The European Common Market and the World* and *Transnational Business Collaboration Among Common Market Countries,* as well as many interesting articles on the subject, has written an excellent and long overdue book on the "external policy" of the European community.

In the preface, Feld sets himself the goal "to examine and to analyze the impact and effects that . . . the European Community (EC) has had and is likely to have on the world's economic and political relations" (p. xi). Accepting Prime Minister Edward Heath's suggestion at the beginning of 1973, that to be able to influence world affairs "Western Europe must speak with a single voice," the author begins the first chapter with an assessment of the "desire of the governments and many people" of the EC members to see Western Europe speak with a single voice in international affairs. He, then, makes a brief evaluation of the power of the enlarged EC. In chapter 2 the author makes a detailed analysis of the "structures and procedures for external policy formulation and implementation." Because of the important implications of the enlargement of the Community for "international interactions," the following chapter analyzes both internal and external aspects of the EC enlargement. In chapter 4, the author takes a close look at the association policy; and, in chapters 5 through 7, he analyzes the impact of Community commercial policies on different areas of the world. Chapter 8 is devoted to the study of international monetary policies followed by the Community members. And, finally, in the concluding chapter, Feld–while drawing upon the discussions in the preceding chapters and keeping in mind the concept of "externalization" developed in connection with neofunctionalist theory of integration by Phillippe Schmitter–assesses the extent to which the EC members' aspirations to "speak with one voice" have materialized. Referring to the foreign policy coordinating mechanism vested in the Political Committee and the extra-Community meetings of the Foreign Ministers of the EC, the author argues that such mechanisms, "reflect the need for externalization as a force toward integration in the sense of Schmitter's definition, but demonstrate that common policies can be adopted outside the Community framework with, initially at least, little benefit for the unification process" (p. 313).

This book seems to be intended especially for those serious students of international affairs who have special interest in European integration. It is a balanced, well-written, documented and, by and large, interesting book. There are, however, some weak points about the book that have to be mentioned. First, the author's account of the reasons for the British decision to apply for Common Market membership in 1961 is not convincing at all. He seems to accept without question Prime Minister Macmillan's statement of August 2, 1961, that "the reasons for the decision to join the Common Market were primarily political" (p. 68). He, of course, concedes that the "economic reasons also played a part in motivating the British decision to seek membership in the Common Market" (p. 69). While not rejecting Macmillan's reasoning and bearing in mind the fact that the borderline between political and economic factors (especially in the international scene) is sometimes very difficult to detect, I believe that economic reasons played a much more important role in changing the British decision to stay out of the EEC than those accepted by the author and expressed by Macmillan on that occasion. (I will discuss in detail the impact of the economic factor in my forthcoming book, *Britain and the Common Market, 1957, 1961.*)

My second criticism concerns the unexplained assertion by the author that "only concerted action by the consumer countries to influence a reduction in the price of crude oil will provide a *satisfactory solution* to the economic problems" (p. 238, italics mine). Here, the reader is almost forced to ask: "satisfactory solution" for whom?

The rest of my criticisms are of quite a different nature. Despite the fact that it should have taken the author quite a time to complete such an extensive study, an effort in publishing it as fast as possible has resulted in several mistakes–at least one of them a major one. On p. 318, *Iran* is the *single example* given for the *Arab* oil-producing countries. On p. 114 and again on p. 255, UNCTAD is referred to as "United Nations Conference on Trade and *Aid*" (italics mine). On p. 157, footnote 15 refers the reader to footnote 38, chapter 3 for names of

ACP countries. Footnote 38 in chapter 3 gives no names.

Despite all this, for the serious student of European integration, there is probably no other single volume that offers as much valuable data—and derived insight—into the nature of the complicated EC external policy formulation and implementation, as well as the impact of the EC on international economic and political relations.

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Yom Kippur and After: The Soviet Union and the Middle East Crisis. By Galia Golan. (New York and London: Cambridge University Press, 1977. Pp. x + 350. $18.95.)

By any measure the Arab-Israeli war of October 1973 was one of the most important international events of the 1970s; some of us consider it to have been the critical turning point of the decade. The appearance of an exhaustive monograph on the immediate circumstances surrounding this conflict is, therefore, unusually welcome. Galia Golan, Director of the Soviet and East European Research Centre of the Hebrew University (Jerusalem) and author of two books on Czechoslovak politics in the 1960s, is eminently qualified. She has provided a month-by-month chronological examination from mid-1972 (Anwar Sadat's dismissal of Soviet advisors) to mid-1974, with virtually an hour-by-hour coverage of the hostilities during October 6-27, 1973. As far as I can determine, every scrap of Soviet published comment is analyzed, together with highly significant revelations by Arab leaders (in Arabic as well as English) and the better-known western sources. Some suggestion of Golan's meticulous documentation is conveyed by noting that the book contains 1,092 footnotes for a textual treatment of 250 pages.

As an indispensable prerequisite for interpretation, a book as thorough as Golan's needs no additional justification. In fact, the work is much more than a compendium. Although at first glance it appears to rely on a simplistic rational-actor model, supplemented by dashes of Kremlinology, I hope to show that the interpretation is more complex. A more serious limitation arises from the strict chronological and topical limits which Golan has set herself. Whether for diplomatic reasons or because of the sheer mass of material she must treat, she virtually eschews examination of the complications of United States policy. As a result, at times her work reads like the reverse image of certain revisionist accounts of the cold war, which reveal immense knowledge of the American side while almost neglecting the opposite side of the superpower equation.

It is hazardous to summarize the findings of such a deliberately restricted and subtly nuanced book as Golan's. Nevertheless, that seems to me the best way to show both its outstanding virtues and its limitations in a brief review. Golan argues persuasively that the primary objective of the Soviet leaders was détente. Contrary to the hesitant encouragement they gave Nasser's aggressive moves before the 1967 war (Golan points out détente was then less formalized), the same Soviet leaders constantly urged caution on Sadat prior to the 1973 hostilities. Yet, while generally aware of Arab intentions, the USSR resumed large arms shipments in early 1973. Similar seemingly contradictory behavior was evident during the war. The Soviet regime consulted with the United States and evidently dreaded superpower escalation. Golan makes it abundantly clear that—despite the U.S. alert of October 24—the USSR never intended large-scale direct intervention, even when the Egyptian Third Army was threatened with annihilation. On the other hand, a massive Soviet airlift (using Yugoslav bases) to resupply the Arab forces was arranged *within three days* after war began. Golan considers that this aid was designed to forestall Arab defeats rather than to accelerate their victories; the timing makes it impossible, I think, to be certain which motive was dominant. Indeed, the whole contradictory pattern just sketched suggests satisficing or incremental adjustments to a dilemma, rather than rational choice of a position between its horns. Golan's interpretation is compatible with the former type of model. The dilemma, which she brilliantly analyzes in her case study, is a persistent one for Soviet foreign policy: how to attract clients among forces aggressively opposed to the status quo while avoiding confrontation with strong powers committed to stability. Having supplied arms to its clients, the Soviet leadership finds it impossible to restrain them, yet fears losses in material and prestige entailed in their defeats almost as much as the risks of escalation involved in saving them from the ultimate consequence of their rashness. Expediencies, repetition of responses used in earlier cases, efforts to have matters both ways rather than choosing between alternatives, not unexpectedly, predominate in these circumstances. Nor is it surprising that certain divergences appear between agencies (predominantly diplomatic) concerned with maintaining détente and others (predominantly military) involved with client support. Conversely, Golan's efforts to trace real policy cleavages in the leadership are unconvincing; it appears incredible to me that, at that late date, A. N. Shelepin could have been pursuing a hard line against the core Politburo members.

When all allowance is made for the incremental, hesitant nature of Soviet policy in the short run, some persistent tendencies stand out. By 1974 its erstwhile intransigent Arab client states were relatively pacified, yet alienated from the USSR. In such circumstances one might have anticipated that a truly incrementalist Soviet leadership would cut its losses in the region, at least for a few years, thus breaking out of the détente-uncontrolled client dilemma. Instead, the Soviet regime turned rapidly to *more intransigent* elements: Iraq, Libya, the P.L.O. Even within the scope of Golan's work, this persistence is striking. Unfortunately, she does not go on to consider the sequence of events—massive military action by North Vietnam, Cuban overseas expeditions, and the like—which accompanied continued Soviet emphasis on détente during the following two years. If she had, more detailed examination of the changing American factor would hardly have been escapable, and a rather more ambitious interpretation of Soviet behavior in changing international environments might have emerged. As it is, her case study provides one of the most solid building blocks yet produced for such an interpretation.

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International Aid and National Decisions: Development Programs in Malawi, Tanzania, and Zambia. By Leon Gordenker. (Princeton, N.J.: Princeton University Press, 1976. Pp. xx + 190. $13.00.)

In the 1960s the operational activities of the United Nations and its specialized agencies expanded sharply. This development has been alternately lauded as an important contribution to world order and condemned as the creation of a type of collective colonialism. Leon Gordenker's book provides a detailed examination of some of these operational activities. Not surprisingly, the evidence he has gathered through careful observation in the field and recounts in clear and graceful prose fails to support either of these doctrinaire interpretations. Instead, the reality of UN technical assistance programs in Malawi, Tanzania, and Zambia is a complicated mosaic.

Gordenker shows that international governmental organizations (IGOs) and their officials had some impact on national policies. National governments sought assistance from the IGOs, but they could only ask for what was available, so the choice was limited from the outset. IGO officials played an important role in the formulation of project proposals, despite the formal doctrine that the UN and its agencies only authorize projects that have been requested by national governments. Some experts provided by IGOs served as high-level officials of national governments (in Malawi such an individual was the economic advisor to the president). Resident representatives of the United Nations Development Program and other IGO officials established close personal relations with national officials. And national officials had other contacts with IGOs, for instance, through conferences and training programs.

Gordenker concludes, however, that the impact of IGOs and their officials on national policies was limited. Multilateral aid programs were modest compared to national development efforts and bilateral assistance. The projects and activities that he observed gave primary emphasis to pragmatic and short-range considerations; little attention was given to doctrines formulated in the resolutions of such organs as the General Assembly and the Economic and Social Council of the UN. Indeed, documents from headquarters were rarely in evidence. When national governments resisted moving in directions advocated by international officials, the national governments prevailed. The projects that he observed were not intimately linked either with each other or with strategies for regional or global development. Decision-making processes both within the field offices of the IGOs and the national governments involved only a small number of persons, and because of frequent personnel changes, patterns of influence were fragile and subject to sudden change.

It is true that the ultimate purpose of much of the technical assistance was to do the necessary preliminary work so as to attract investment. This could be regarded as perpetuating and promoting the capitalist world system, but the preliminary work had to be done whether the ultimate investment was public or private. It was assumed that much of the actual investment would be done by the International Bank for Reconstruction and Development, but Gordenker shows that resident representatives often had little knowledge of the bank or contact with it. Nor did they have much training in economics.

Skeptical of the theories of desk-bound scholars and of the antiseptic accounts of official reports, Gordenker set out to observe the operational activities of the UN and the specialized agencies at close hand. His sensitive description is an essential antidote to earlier portrayals.

But Gordenker's preference for hewing close to empirical reality and eschewing theory has limitations. The absence of a conceptual framework makes comparisons difficult, and consequently he is unable to generalize about factors that contributed to the success or failure of projects. Indeed, he does not venture a clear definition of success.

Early in the book, Gordenker postulates that IGOs "will become ever more deeply involved in the political decisions of their members in the future" (p. 20). The evidence that he presents, however, would seem to contradict this idea. In East Africa resident representatives shifted toward a narrower and more administrative conception of their role, and the experts who served in high-level positions in national governments were gradually replaced by national officials. The entire issue unfortunately is impenetrable because the author does not provide benchmarks for measurement.

Thus questions about the operational programs of the UN and its specialized agencies that are currently debated by policy makers and scholars cannot be answered by this book. This does not mean that one should not read it. Those interested in IGOs and development must read it, and they will profit from it, though perhaps not as much as they would wish.

HAROLD K. JACOBSON

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The World and the Great-Power Triangles. Edited by William E. Griffith. (Cambridge, Mass.: MIT Press, 1975. Pp. 469. $19.95.)

The purpose of this volume is to explain how the rigid post-war alliance structure evolved into a multipolar system, and how these changes affect the international system. These objectives are only partly achieved: the case studies are rich in detail and filled with interesting, even brilliant, insights; but the study lacks unity, and offers no analytical continuity or coherent, general findings.

William Griffith is obviously a very knowledgeable and capable specialist on matters treated in this study. He argues convincingly that the interaction between the political-military triangle comprised of the United States, the Soviet Union and China, and the economic triangle composed of the United States, Western Europe, and Japan should be used to evaluate changes in the international system. Yet, he does not exercise much care in setting up the framework. The dynamic interaction between triangles is not explored, the conditions for change are not developed, the consequences of membership are vague, and, most importantly, the triangular framework is not tested in the case studies.

Griffith implies that members of both triangles are superpowers and they play a central role in reconstructing alliances. Obviously, the definition is too narrow since only the United States qualifies as a superpower. More importantly, Griffith does not follow a single consistent criterion to establish superpower status. For example, China is a member of the military-political triangle (p. 1), it is strong enough to deter the Soviet Union (p. 30), and like the Soviet Union, China is not an economic superpower because of their autarkic economic system (p. 1). But, inexplicably, the Soviet Union is a superpower and China is not. Moreover, Griffith argues that "Japan, not China, is the only potential third superpower" (p. 11) because Japan can develop military capacity while China cannot modernize its economy. I, for one, remain skeptical of such unsupported arguments; but, more importantly, this evaluation devastates the analytical value of the triangles. If most of the nations in the two triangles cannot change their status, structural changes in alliances cannot be attributed to variations in the membership of the triangles. Clearly, constants explain nothing.

Even after the superpowers are defined, new constraints weaken the utility of the triangles. Members of the triangles are not necessarily influential. For example, the United States lacks the crucial "will to power" and cannot take advantage of its strength (p. 28); Japan is inhibited by psychological considerations that prevent the development of military capabilities (p. 29). If Griffith is correct, and will or psychology play such an important role in power, why are these factors excluded from the triangles? If influence does not follow from political, military and economic capacity, if the international system is not altered by the superpowers, why is the triangular framework advanced in the first place? What can we explain using this information?

Griffith does not provide the answers. Rather than presenting hypotheses that could be scrutinized in the case studies, he advances deterministic propositions that cannot be tested. For example, Griffith argues that "Soviet-American hostility after 1945 was in my view structural and inevitable" (p. 3). Evidence can be summoned to support these propositions. But were they inevitable? Were they related in any way to changes in the power triangles? One cannot decide using the evidence provided in this volume. The case studies do not explore superpower interaction directly; rather, they evaluate the foreign policy of Japan, Korea, India, Egypt, Israel and selected Eastern European nations. The relation of these studies to the original framework is weak.

The best study is Paul Langer's contribution that skillfully places Japanese foreign policy within the context of the relations between the United States, the Soviet Union and China. He relates domestic mass attitudes to changes in Japanese foreign policy and indicates the difficulties of the changing to a course less dependent on the United States. He spends little time on individual leaders; rather he lucidly and persuasively discusses the aggregate interaction between the major powers and evaluates the effects on changing alliances. This excellent contribution comes closest to the stated aim of the book.

Victor Meiser traces the political dynamics in the Balkans and provides a succinct and interesting historical overview of Romanian and Yugoslav domestic politics. In a separate section he evaluates the foreign policy of these nations and of Albania and Bulgaria. The extensive background material is not directly connected to the foreign policy material or to the overall power triangle thesis.

Arnold Hottinger's chapter on the Middle East provides an authoritative, detailed, almost daily description of the decision-making process in the Middle East. Major targets are the decisions of Sadat and before him, Nasser. The domestic interplay between the leaders in the Middle East is evaluated with care and competence but, the superpowers–in this case the Soviet Union and the United States–are considered marginal, constraining actors rather than the main players. Thus, Hottinger attributes little importance to the interplay of power triangles.

Bhabani Sen Gupta provides a rare and detailed evaluation of the interaction between India and its neighbors during the creation of Bangladesh. This competent, well-written description of regional politics stresses the importance of regional as opposed to global intervention to resolve conflicts. The central argument is that India advanced its interests in the region unconstrained by the major power triangles.

Chong-Sik Lee lucidly discusses the changing relations between North and South Korea after the United States renewed relations with China. He evaluates the interplay of personalities in the peninsula and analyzes the subtle changes in domestic and regional politics that led to relative détente in the area. He argues persuasively that changes in great power relations made renewed talks possible in Korea. Again, major powers provide the conditions for change but are not central actors motivating restructuring in the region.

This book fulfills part of its promise. It is a valuable contribution because of the careful evaluation of foreign policy in areas seldom covered, but it fails to provide a viable framework, common set of testable hypothesis and a general set of findings.

JACEK KUGLER

Boston University

Japan, America, and the Future World Orders. Edited by Morton A. Kaplan and Kinhide Mushakoji. (New York: Free Press, 1976. Pp. 369. $12.95.)

This book, containing 17 essays by 22 authors, is an outgrowth of two conferences on Japanese-American relations held in Chicago and Tokyo in 1973 and 1974. The topics covered include the following: Sino-Japanese relations (Tsou, Najita, Otake, Hsiao); self-images of the mass public concerning the role of Japan and the United States as international actors (Watanuki); internal decision-making processes (Hosoya, Destler, Ori); Japan's role in the world economy and its economic relations with the United States and Southeast Asia (Hamada, Scalera, Kahn, Aliber, Ginsburg, Osburn, Kawata); environmental constraints on Japan in the international system (Royama); the role of science and technology and its impact on U.S.-Japanese relations (Imai); perceptions of Japan held by "foreign policy publics" in the United States and their implications for intercultural relations (Iriye, Harris); and interrelationships among the economic systems of China and those of other Asian countries (Nakagane).

The opening essay by Kaplan is a refreshing and perceptive analysis–relatively unencumbered by the perspective of an area specialist–of Japan's vital interests, constraints on its future, the nuclear issue, and the prospects of Japanese-American relations. Mushakoji's four proto-scenarios on the trilateral interaction between the United States, Japan, and the East Asian countries are in need of sharper conceptualization, with their utility–whether as a heuristic device for understanding, predicting or constructing theory–yet to be demonstrated. Tsou, Najita, and Otake argue persuasively that there is little prospect of intense Sino-Japanese rivalry. Systematic attention to the role of the internal setting factors of each country would have enhanced the quality of their analysis.

Hsiao gives a highly informative account of several external constraints operating on the development of Sino-Japanese relations. His discussion of the "Sino-Japanese nuclear force supported by the U.S." is provocative. In his view, the establishment of a joint Sino-Japanese nuclear force could strengthen the defenses of each nation and serve as a counterweight and deterrent to the superpowers, thus reducing the danger of nuclear war. He observes that Chinese cooperation or support for a U.S.-Japanese nuclear force will be obtainable under specific circumstances and alludes to the possibility of a joint U.S.-Japanese-Chinese nuclear force.

In view of the Soviet Union's extraordinary sensitivities and the capabilities and options available to it, the risks to the three countries accompanying a movement toward such a joint force might be disproportionately greater than the advantages that might accrue. Hsiao's idea presupposes a fundamental and radical transformation in the existing domestic political alignments in the countries concerned and in the structure and patterns of interaction in international politics.

That the topic of Japanese-Soviet relations was not given full, chapter-length treatment is strange in view of the frequent explicit and implicit references to it made by a number of contributors. Hsiao provides a brief, yet useful, account of Japanese-Soviet relations as a factor constraining Japanese-Chinese relations. I think Hsiao underestimates the intensity with which the Soviet Union desires Japan's economic cooperation. It is misleading to ascribe the lack of progress in negotiations concerning certain economic projects to Japan's unwillingness to support the Soviet proposal for a collective security system in Asia. There is little linkage between these economic projects and the Societ proposal, though linkage probably exists between the projects on the one hand and the question of Northern territories and a peace treaty on the other.

I do not share Hsiao's optimism that the Soviet Union might yield to Japanese territorial demands in return to Japan's acceptance of a Soviet collective security system in Asia. There are mighty reasons for Soviet reluctance to relinquish these territories, and Japan's support for the concept would be wholly inadequate compensation. Japan's very refusal to support the Soviet proposal is owing, in part, to her perception that it would have an adverse implication for her territorial demands. Soviets, too, are conscious of the same consideration.

Destler's chapter on the influence of American Japan experts on United States foreign policy is full of insights. His discussion of the general policy-making process within which American Japan experts must seek to exert their influence is followed by an incisive analysis of the decline in the importance of these experts during the Kennedy, Johnson, and Nixon years. A more comprehensive treatment of general factors shaping United States policies toward Japan would have been particularly valuable.

Ori's article represents primarily an effort at distillation resulting in a useful, albeit descriptive, account of political determinants of selected foreign policy decisions. Ori observes that, beyond case studies, no systematic study of the relationship between Japan's political structure and its foreign policy has been made. What is needed, I think, is a series of systematic, empirical inquiries preceded by: (1) the development of theoretically oriented concepts (more refined categories than the four determinants Ori identifies) and their empirical indicators, and (2) the construction of a pretheory specifying the interrelationship among the relevant variables and specifying the circumstances under which the variables would assume different weights.

The remaining chapters, too, contain a wealth of descriptive information, insights, and incisive analyses. Both Japan area specialists and the general reader will find the book highly informative, stimulating and indeed worthwhile.

YOUNG C. KIM

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The Economics of Defence. By Gavin Kennedy. (Totowa, N.J.: Rowman and Littlefield, 1975. Pp. 251. $17.50.)

This is a modest little book by an English economist. It is literate and sensible in mapping and clarifying economic concepts. It demonstrates the need for elementary clarity with examples showing where prestigious scholarly commentators on defense issues have been sloppy and inaccurate. It will be useful to the teacher, but scarcely to the expert. For example, chapter 2, on the economic theory of public good, is clear and readable but scarcely scratches the surface of the subject or the bibliography of defense or military public goods. Kennedy began where he should have; his priorities are right; but one might have hoped for more persistence or reach. Yet modest books have their place, this one included.

Kennedy remains at his best on literary macro-economics, while forays into related defense subjects are less successful. A fine chapter on defense and industry competently refutes some of the sillier charges about the military-industrial complex. The more serious charges deal with subtle modes of influence, subtle biases, and indirect results, charges which lie beyond the reach of this book. Another example of Kennedy's sensible exposition that does not probe deeply is his handling of disarmament. Confining himself mainly to the economics of disarmament, as in his last chapter, he has little to say beyond the fact that disarmament need impose no more than passing troubles for a national economy, a point that he had already made when dealing with the military-industrial complex. It is no longer of much value to discuss disarmament in the general economic terms of this chapter, except for the convenience of the reader.

When Kennedy goes more deeply into defense subjects, he is less successful. His discussion of British defense budgets reads like a Rand Corporation sermon on program budgeting, c. 1959. He deals in American publications, yet ignores the explosion of questions that followed the imposition of program budgeting in the Defense Department in the early 1960s. To take this chapter as a statement about British defense budgeting, which it appears to be, is to raise many questions that are not here addressed, such as the following: (1) American defense analysts attributed the existence of narrowly conceived accounting budgets to the role of Congress in the separation of powers. Why then do the British seem to have had the same problem? Have there been no comparable budget reforms in the U.K. since 1960? (2) If not, have British defense analysts and managers been ignorant of the problems of data reliability and conceptual structuring that now have come to be understood as inherent in program budgets and other management information systems? (3) What has kept the British so ignorant of American developments and problems?

Kennedy has ignored the American experience (the pros and cons of program budgeting) as it has been described and evaluated since the mid-sixties; he has read and digested the literature up to the mid-sixties on defense analysis and defense management. The result is not surprising: a quaint enthusiasm about the fads of American policy analysis in that period. (Later fads are not mentioned.)

In his discussions of British defense budgets, weapons development and defense planning (three of nine chapter headings), Kennedy is an advocate of systems analysis. Again, this is vintage stuff. It does not show the self-criticism and qualifications that entered the literature of systems analysis beginning in the mid-sixties. Kennedy has absorbed the self-criticisms that deal with analytical process—the need to iterate analytical procedures, for example—but not the criticisms that arise from observing the actual uses of systems analysis. For example, there is reference to the problem of quantification, but no serious concern with the persistently observed fact on the American scene that nonquantified factors tend to get ignored.

There is a simple explanation for all of this. Footnotes reveal that Kennedy has taken very seriously the literature of American defense analysis—that is, defense analysis by economists—through the mid-sixties and that he has taken note of Hitch's and Enthoven's reminiscences of the period, which were published later. He has ignored important literature of criticism and reappraisal since the mid-sixties. The result is a peculiar combination of clearheaded general economic exposition and vintage American defense analysis.

In 1952 an American graduate student cut the pages on the single library copy at the London School of Economics of Pendleton Herring's pioneering prewar book on civil-military relations. That this volume had been so neglected for that decade seemed an appalling indicator of indifference among English scholars to a subject that deserved their serious attention (and was about to get it). In the light of Gavin Kennedy's book, perhaps that judgment, or part of it, was mistaken. It still seems obvious that British scholars ought to pay serious attention to major public policy issues, defense policy included. But perhaps they are better off not reading Americans so much.

For the teacher who is interested in examining with his students national perspectives on defense policy, though, Gavin Kennedy's book may be a useful special purpose book for courses in policy studies and defense policy.

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Power and Interdependence: World Politics in Transition. By Robert O. Keohane and Joseph S. Nye. (Boston: Little, Brown, 1977. Pp. x + 268. $11.95, cloth; $5.95, paper.)

In 1971 Robert Keohane and Joseph Nye edited a special issue of *International Organization* on transnational relations and world politics. Together with other contributors to that volume, Keohane and Nye questioned the conventional scholarly wisdom regarding the role of nonstate actors in world politics, the role of nonmilitary techniques of statecraft, and the role of economic variables in the international system. The timing could not have been better. Both public and scholarly interest in economic dimensions of international politics was growing rapidly even before the oil crisis of 1973 captured everyone's attention. (Even Henry Kissinger began to take international economic problems seriously.) Having identified a number of weaknesses in state-centric, "realistic" theories of international politics, the authors devoted themselves to an attempt to provide an alternative theoretical approach. *Power and Interdependence* culminates that collaborative effort in an impressive way. Suffice it to say that every serious student of international relations—not just international political economists—must read and come to terms with this book.

The authors address two major questions: first, "What are the major features of world politics when interdependence, particularly economic interdependence, is extensive?" And second, "How and why do international regimes change?" (p. 5) The very question is answered by delineating two fundamentally different characterizations of the international system, "realism" and "complex interdependence." These alternative characterizations are defined in terms of three basic dimensions: (1) the extent to which states, acting as coherent units, are the dominant actors, (2) the extent to which military force is a feasible and effective instrument of statecraft, and (3) the extent to which national military security dominates foreign policy agendas. Under conditions of "complex interdependence" national societies interact through multiple channels, foreign policy agendas consist of "multiple issues that are not arranged in a clear or consistent hierarchy," and military force is not very useful (pp. 24-25). Both realism and complex interdependence are described as "ideal types," representing two ends of a spectrum with most real-world situations falling "somewhere between the two extremes" (p. 24).

The question of how and why international regimes change is answered by comparing the explanatory power of "four models based respectively on changes in (1) economic processes, (2) the overall power structure in the world, (3) the power structure within issue areas, and (4) power capabilities as affected by international organization" (p. 38). The respective models become progressively more complex, and some situations require various combinations of the models in order to provide a "full explanation." These four models are applied to four case studies concerning international monetary relations, ocean politics, Canadian-American relations, and Australian-American relations. The cases were chosen more for theoretical reasons than for their intrinsic interest. Potential readers should not let a lack of interest in the particular cases deter them. The case studies provide a useful means for the authors to explain, illustrate, and test their theoretical ideas. Although adding to our factual knowledge of the specific cases is not the authors' primary purose, it is a significant side effect. (American readers are guaranteed to improve their factual knowledge about Australian-American relations!)

Although the strong points of this book are too numerous to mention, a few salient ones should be noted. First, the authors provide an effective demonstration of the limited utility of traditional theories of international politics in a world that increasingly approximates the conditions of complex interdependence. As Graham Allison demonstrated with regard to the Cuban missile crisis, examining a given case using different explanatory models is an excellent way to drive home the point that theories matter, even to those who claim to be interested only in practical policy questions.

A second strong point—and in this reviewer's opinion, the most valuable contribution of the book—is the effective demonstration that in political analysis *scope matters,* i.e., power varies from one issue area to another. Others, including Harold and Margaret Sprout, Robert Dahl, Harold Lasswell and Abraham Kaplan, and Karl Deutsch, have made this point; but no one has provided such impressive evidence as to why students of international politics should take it seriously. Those who try to explain world politics with systemic theories have been especially oblivious to the importance of issue areas. Nye and Keohane demonstrate that a systemic approach that takes account of different issue areas can be very useful.

A third strong point of the book is the balance between data and theory. The authors avoid pointless quantification and emphasize theory; but they have made a serious attempt to seek relevant empirical evidence whenever possible and to present it so as to facilitate the reader's understanding of their argument.

Fourth, objectivity, scholarly caution, balanced judgment, and intellectual humility infuse the book. (Perhaps this is partially due to the comments by 53 leading scholars [p. x] received by the authors before publication). At times, scholarly caution even approaches scholarly timidity. In their attempt to be fair, the authors may overestimate the continued relevance of traditional views of world politics; and in their intellectual humility, they may underestimate the irreversibility of current trends toward complex interdependence. Still, objectivity and humility are reasons to praise rather than criticize this book.

In view of the impressive array of scholars that have commented on the manuscript, it is probably both presumptuous and futile to try to find fault with this book. However, a book as important as this one ought to be free from the petty cost-cutting exemplified by placing the footnotes at the back of the book and the omission of a bibliography. The index is barely adequate and carelessly done—as I discovered when trying to check references to the International Bank.

Also the authors have a disconcerting tendency to imply that explanations can be classified as "adequate" or "inadequate," "sufficient or insufficient," "satisfactory or unsatisfactory" in a given situation (e.g., pp. 4, 58–59, 224) without reference to the purposes of the analyst. Thus, although they point out that the varying conditions of world politics and the varying issue areas require use of more than one model, they do not seem to recognize that the "adequacy" of a given explanation of why the Bretton Woods international monetary regime broke down depends partly on *why* one wants to know. Explanations that they find inadequate for their purposes may be more than adequate for the purposes of other analysts. (When my five-year old asks me what time it is, I usually round to the nearest half-hour; but when a student asks for the time during an hour exam, I try to round to the nearest minute.)

Perhaps the least satisfactory aspect of the book is the treatment of the two central concepts on which it is based—"power" and "interdependence." If one's argument rests heavily on two concepts as closely related as "power" and "interdependence," it is essential to be unmistakably clear in defining those terms and in distinguishing them from one another. Otherwise, the reader cannot be sure whether statements about interdependent relationships serving as power resources are tautological or not. Perhaps it is due to my own density, but after several readings of the passages on power and interdependence (pp. 8–19, 224–26), I am still not sure whether such statements are tautological. If the authors had indicated their awareness of the risks of tautology in discussing power and interdependence or if they had cited some relevant theoretical literature (e.g., Peter Blau, *Exchange and Power in Social Life* [New York: Wiley, 1964]; or Richard Emerson, "Power-Dependence Relations," *American Sociological Review* 27: 31–41), I would have been somewhat reassured on this point.

The treatment of power is uneven in precision and sophistication. Although it is clear that the authors do not believe that military force is always more effective than other techniques of statecraft and that they do believe that power varies from one issue area to another, at times they give the opposite impression. For example, if power varies with issue areas, it does not make much sense to use such terms as "Great Powers," "Small Powers," "strong states," "weak states," or "the power structure," since these terms have habitually been used to imply generalized rather than issue-specific power. Hans Morgenthau may talk that way, but Robert Dahl and Harold Sprout do not. It is also disconcerting to be told that military power "dominates" economic power or that "power resources provided by military interdependence dominate those provided by nonmilitary" interdependence (pp. 16–17). Cost clearly limits the utility of military power in the view of Keohane and Nye, but it is less obvious that they perceive the conceptual impossibility of making any statement whatever about the relative effectiveness of military power without first specifying what the Sprouts have called "the policy-contingency framework," i.e., assumptions as to who is undertaking what, against whom, and under what conditions (Harold and Margaret Sprout, *The Ecological Perspective on Human Affairs* [Princeton: Princeton University Press, 1965]; and *Toward a Politics of the Planet Earth* [New York: Van Nostrand, 1971]). Knowledge of the power structure (distribution of capabilities) is not and cannot be "the best starting point for policy analysis," as Keohane and Nye claim (p. 225), because knowledge of the power structure *presupposes* assumptions about what policies nations are likely to undertake against what opposition and under what conditions. One cannot even identify "power resources" except in the context of a policy-contingency framework. Until placed in a policy-contingency framework, uranium is just dirt; and petroleum is just a rather messy liquid. Once again, the paucity of references to the theoretical literature on power strengthens my misgivings about the conceptual treatment of power in this book. For clarity, precision, and sophistication in the conceptual treatment of power and interdependence, Klaus Knorr's *The Power of Nations* (New York: Basic Books, 1975) is preferable. Fortunately, *Power and Interdependence* is better than its conceptual underpinnings and provides us with an impressive example of imaginative and enlightening discussion of power and interdependence in today's world. Keohane and Nye are better at using these concepts than they are at explaining them. It is one thing to be able to play the piano well and another to be able to explain how one does it.

In sum, despite its conceptual shortcomings, this is an outstanding scholarly contribution. If you intend to read only one international relations book this year, make it this one.

DAVID A. BALDWIN

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The War Managers. By Douglas Kinnard. (Hanover, N.H.: University Press of New England, 1977. Pp. ix + 216. $14.00.)

The basic premise underlying Douglas Kinnard's reassessment of the Vietnam War is very appealing. Unlike Samuel Huntington, who advises policy makers to "simply blot out of their minds" any recollection of America's Vietnam misadventure, Kinnard believes that "we owe it to ourselves and future policymakers to get a better grasp on what went wrong" (p. viii). Toward that end, Kinnard (a political scientist as well as a former brigadier general in Vietnam) has surveyed the opinions of some 110 other army general officers who held command positions in Vietnam between 1965 and 1972, has analyzed public documents pertaining to the American military involvement, has combed through certain files in the Army Center of Military History, and has conducted in-depth interviews with a selected group of generals.

Kinnard's "mission" in *The War Managers* is no less ambitious than his multifaceted data-gathering effort would suggest. Within one slim volume, Kinnard attempts among other things, to deliver a chronicle of American involvement, to interpret the political and military objectives of the United States and the North and South Vietnamese, to assess the military strategies, tactics, and organization of the contending parties, to compare the American military's performance in Vietnam with its role in other recent conflicts, to analyze the role of the news media in reporting on Vietnam, and to spell out the "lessons" of Vietnam. Ironically, the primary problem with this book is that Kinnard himself succumbs to the very same failing for which he is justly critical of U.S. policy makers: lack of clarity concerning objectives. The book, brief as it is, simply attempts too much. As a result, the real contributions that Kinnard makes are all too often camouflaged by superficial glosses of matters that are dealt with better elsewhere. For example, serious scholars—or for that matter anyone who paid even passing attention to the news between 1964 and 1975—will learn little from Kinnard's compressed treatment of events in Vietnam, an aspect of this work that might better have been either entirely omitted or compressed even further into a simple time-line to which interested readers could refer. The sections on military strategies, tactics, and organization are much stronger, easily the highlight of the book. It is here that Kinnard comes closest to providing fresh insights, probably because it is here that Kinnard and his generals are dealing with questions that they are perhaps uniquely qualified to answer. Kinnard reveals, for instance, that almost 70 percent of the generals professed to be uncertain of the war's objectives, which must have made their tactical decision making an exercise in bewildered ad-hocracy. The generals' frank assessments of the military capabilities of the Viet Cong and the North Vietnamese, of the "measurement of progress" system of statistical reporting, of inter-service cooperation, and of the military expertise of ARVN general officer leadership, all add to our knowledge of what went wrong in Vietnam.

Unfortunately, there is far less to be learned from Kinnard's generals when they stray outside their area of special expertise—the waging of war—to confront complex social and political issues. The generals do not, for example, seem to have appreciated American newspaper and television coverage, seeing it as "counterproductive to the war effort." That judgment may or may not be accurate, but one wonders why Kinnard posed such questions in the first place. Asking war managers to comment on trends in mass communication and public opinion seems roughly equivalent to asking Louis Harris or George Gallup what they think about military tactics; such answers may tell us something about the mind-sets of those who give them, but they convey precious little about the substance of the question.

Nor do the generals—or Kinnard himself, for that matter—seem entirely certain about what to make of the American experience in Vietnam. In a very brief concluding section, Kinnard struggles vainly for some useful lessons. And the generals? As of September, 1974, when Kinnard conducted his survey, only 53 percent had a negative retrospective assessment of the U.S. combat role in Southeast Asia! (p. 179) In sum, Kinnard's treatment suffers when he or the generals strive for The Big Picture. The book does, however, have its strengths, and these are most evident when Kinnard's aim is most modest and his focus most restricted.

LEE SIGELMAN

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Historical Dimensions of National Security Problems. Edited by Klaus Knorr. (Lawrence: University Press of Kansas, 1976. Pp. 387. $6.95, paper.)

Historical Dimensions of National Security Problems is the sixth in the National Security Education Program Series under the general editorship of Frank N. Trager of New York University. This volume, edited by Klaus Knorr, exhibits the anxieties felt by some scholars in the field of national security and international relations and is designed to bring about an end to the present neglect of history. The fear is expressed that the past ten years may have precluded the consultation of historical materials and may truly reflect a "counterculture" with a positive antihistorical impetus. The editor is concerned that some social scientists "often trained in little else than statistical method" (p. 2) may claim a kind of transtemporal validity by "quantifying what in most instances resists quantification" (p. 2). In the editor's view, they do not study history; they "ransack" it as they seek data to support their hypotheses.

In the search for the "constants" of history which the authors feel are useful for a comprehension of the present, the articles range over such topics as morality, technology, alliances, threat perception, civilian supremacy, and mental constructs for viewing international affairs. There are eight essays; "What Happened? The Problem of Causation in International Affairs" by Allan R. Millett and William B. Moreland; "Military Strategy and Civilian Leadership" by Russell F. Weigley; "Threat Perception" by Klaus Knorr; "Response to Threat Perception: Accommodation as a Special Case" by Peter Karsten; "War-Limiting" by Charles H. Fairbanks; "Alliances, 1815–1945: Weapons of Power and Tools of Management" by Paul W. Schroeder; "Technological Change, Strategic Doctrine, and Political Outcomes" by Bernard Brodie; and "The Moral Basis of National Security: Four Historical Perspectives" by Thomas L. Pangle.

Each of the essays is a tour de force by itself, particularly, Thomas Pangle's essay which is almost a brief course in political philosophy. In a far-reaching search for the moral justification of our understanding of the term "national interest," he explores the moral basis of foreign policy and war from Greek thought to the modern idealism of Immanuel Kant, who he believes placed a greater importance on international affairs than "any previous thinker."

A most innovative and challenging idea is presented in Weigley's essay, in which he suggests that Clausewitz has been misread in the cliche that "war is the continuation of diplomacy by other means." War, Weigley feels, may develop its own crushing logic and defy civilian control because war is too closely bound up with the nature of the state itself. The idea underlying the state, Weigley attributes to Clausewitz, is "defense against the enemy without" (p. 70). War, he believes, lends a demonaic quality to all statecraft, and threatens to design its own future, to exceed all national policy-making restraints.

This expanding vortex of war concept is also explored by Charles Fairbanks in his pessimistic war-limiting essay on the Italo-Turkish limited war of 1911–1912, in which he concludes that modern liberal democracies may "lack the intellectual basis for acceptance of war-limiting" (p. 202) and may produce unwillingness to fight or project democracies into total war with unlimited goals. To one who takes the collective wisdom of both Weigley and Fairbanks, the chances for imposing either the limitations of civilian control over war objectives or expecting public outcry to restrict wars appear quite dim.

Paul Schroeder's explanation of alliances as involving the "tools of management for the purpose of promoting international peace and stability" (p. 256) irrigates the barren sands repetitiously sifted that explain alliances strictly from the viewpoint of friends working with friends. His thought is that these devices unite rivals and can be best understood as associative-antagonistic relationships. Thus the revisionist-classicist argument over the origins of the cold war can be better appreciated as a U.S. effort to manage the Soviet Union and return the wartime ally to the U.S. concept of the alliance goals. By interjecting the "hostile-allies" concept between the balance of power versus international community schools of thought Schroeder has contributed a new vista for viewing the conceptual constructs we use in this arena.

The essays by Klaus Knorr and Peter Karsten on "Threat Perception" seem to suggest that we have a long way to go before this dimension of the state actors can be used to achieve a level where either predictability or the creation of guiding principles can be assured. Karsten seemed to say it best when he stated, "If we have not revealed any dazzling new laws of international relations, . . . we can probably agree that the historical record is the place to begin in searching for such principles" (p. 159).

Millet and Moreland suggest some mental constructs that could be useful for analyzing international affairs, such as (1) "rational actor, (2) organizations as actors, (3) individuals in bureaucratic setting, and (4) domestic politics and social events." While there is not much new in this approach, the combining of the constructs is useful.

Finally, Bernard Brodie's treatment of technological change is laced with facts and should be required reading for any basic course in international relations that deals with the impact of technology and weapon systems on national policy.

Knorr has succeeded in his declared purpose, to bring historical knowledge into direct contact with our immediate world problems. This collection of essays may also accomplish his secondary purpose, that is to suggest some of the "constants" of history and to draw from them their relevance to the present-day world. History is indeed a cruel master if we cannot learn from it, and we may indeed be forced to repeat its mistakes.

CHARLES A. RAITH

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Law and Politics in the International Society. By Werner Levi. (Beverly Hills, Calif.: Sage, 1976. Pp. 191. $11.00, cloth; $6.00, paper.)

Werner Levi has undertaken in this volume an analytical task for international law similar to that which he performed for international organization in *Fundamentals of World Organization* more than a quarter of a century ago. At that time, Levi responded to the spate of historical and descriptive studies by investigating international organization on the basis of the human foundation upon which it rested. Now his analytical skills and unflagging spirit are turned toward a subject which is often viewed by political scientists, albeit mistakenly, as stuffy, dull, rule-oriented, anachronistic, irrelevant and nonpolitical. The problem is not international law itself; rather it is the narrow legalistic teaching of international law. The pressing need is for an approach which solidly installs international law as a vital, integral policy component of the real world of international politics.

Levi recognizes this fact when he writes that "the nature of international law is interpreted as a dependent of the nature of international politics. Both are assumed to perform social functions and to be creations of men" (p. 7). In the same vein, he further elaborates, ". . . the admonition of the politician or the lawyer not to mix up politics and law can only be an attempt, usually suspect, by both not to be strained in the exercise of their particular functions. The two are inseparable, and neither can be understood or properly practiced without regard to the other" (p. 31).

Many share this notion, but few go to such lengths as Levi has done in subjecting international law and politics to close scrutiny within a viable and appropriate framework, in his case a social foundation perspective. His general orientation goes something like this: Men and women must survive collectively if they want to survive individually. Only social action can produce the type of social behavior which provides regularity and predictability. Within this system, the function of law is to create social order by commanding requisite behavior. In essence, Levi examines the broad field of law within the complexities of international society and its human foundation and more particularly within the context of the society's politics.

Specifically, Levi analyzes the nature and function of domestic and international law as they relate to the concepts of national sovereignty, political power and political processes, and to the general topics of sources and subjects of international law, state equality and inequality, cultural heterogeneity and international conflict. He disclaims any preconceived theory of politics or law but his attachment to power politics is by no slender thread. The state is supreme and power is dominant. Nevertheless, Levi emerges with an overview of a weak but surging legal system.

Some may grumble at the sharp distinction he draws between law and politics, but few would quarrel with the law-politics integration and the mutual dependence of the two. Some legalists, and even political scientists, may quibble over his social framework, but few would disagree that law must be wrested from its legalistic domination if it is to become a viable subject of study for political science.

The link with power and the skepticism which sometimes permeates the analysis might nettle those of us who cling to a more hopeful view of the world order. But Levi notes that some elements of power have lost much of their essentiality and observes: "The state of the world provides no ground for undue optimism. But looking at international law as it was some decades ago and as it is today permits the expectations that mankind will manage to survive, which it can do only with the help of law" (p. 171). That view is taken by others who have dealt with the interaction between international law and politics. But Levi adds a new dimension. He places each of his topics within the social foundation setting, providing a springboard for reorientation of the study.

Happily, Levi's book appears at a most opportune time, since international law appears to be on the threshold of a new era in which it will shuck its heavy legal trappings. And the law-politics integrative message comes from a scholar who is neither an apologist for nor a denier of international law. The wide-ranging documentation from sociology, law and politics reflects the richness of Levi's own background and intellectual insights. His *Law and Politics in the International Society* is a fitting companion to his well-received works on organization and politics and deserves attention, not only from international law specialists who toil in the dimmer reaches of political science, but even more so from the experts in international politics whose heady mainstream role too often blinds them to the realities of the global legal-political systems in which they dwell.

CLIFTON E. WILSON

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Quest for Equilibrium: America and the Balance of Power on Land and Sea. By George Liska. (Baltimore: Johns Hopkins University Press, 1977. Pp. xxii + 254. $14.00.)

George Liska writes in the classic tradition of the "realist" school of international politics wherein the balance of power is the central theoretical precept. According to Liska, American foreign policy has exhibited three tendencies: the two basic strategies of empire and equilibrium, and the vain (unrealistic) desire to transcend international politics altogether. This volume concentrates on the strategy of equilibrium. A forthcoming volume will focus on empire.

Liska's method of analysis is the historical parallel (he calls it "analogico-historical," for what he claims is "lack of a simpler term," p. ix, but the reader soon discovers that Liska is not inclined to choose simple terms nor is he likely to clarify his concepts with immediate and parsimonious definition). *Quest for Equilibrium* thus accounts for the rise of the United States to global preeminence in terms of historical parallels, primarily of land-sea rivals of the European ("Eurocentric") international system. For example: "to a large extent, Soviet Russia is the Imperial Germany of the earlier era, just as the United States is the British Empire, while Western Europe is erstwhile France, and China replicates Tsarist Russia" (p. xix). Alternating between cycles of empire and equilibrium, the United States emerged from World War II to build and sustain imperial power in conjunction with the cold war, and when that began to wane, after Vietnam, to seek an effective balance of power (equilibrium quest) in which the choices are three: a bipolar balance, or "parity" arrangement between the United States and the Soviet Union; an equilibrium of manipulated multipolar balances of power in conjunction with the cold war, and major powers cooperate to prevent radical disorder emanating from the third world.

Liska's focus remains consistently macroanalytical. States move on the chess board of international politics as unitary actors seeking goals largely determined by international systemic requisites. Configurations of states produce both regulatory mechanisms and disruptions as rising and declining rivals are more or less effectively integrated into the system. States appear to have choices and they pursue strategies, but they are critically restrained by or shaped by the structure of the system. Liska is particularly impressed by the inherent advantages and disadvantages of maritime and continental powers: "just as [i.e., like] dogs and sharks, major land and sea powers cannot destroy one another in their respective elements" (p. 28). The basic "schism" between land and sea power is illustrated by such historical parallels as Venice and Italy, Great Britain and France (and, later, Germany), Japan and China, and so on.

A problem, of course, in using this particular parallel as a means of understanding the rivalry between the United States and the ascendant continental power, the Soviet Union, is that, first, the United States is both a continental and a sea power; and second, modern means of transport and communication make the land-sea schism less salient. While Liska acknowledges these differences, his concessions to these points do not deter him from heavy use of this historical parallel.

Quest for Equilibrium lacks an explicit theoretical framework that would prepare the reader for the conceptual trials of the book. Descriptive and explanatory statements are intermixed throughout, underpinned (usually by implication) by that eternal verity, the balance of power. Theoretical propositions concerning the functioning of alliances, the balance of power processes, the differing interests of maritime and continental powers, the tendencies of satisfied and dissatisfied, of rising and declining, of challenging and defending states, are scattered throughout the text. As we encounter these concepts, our confusion is increased by both an awkward style and the absence of simple clarification.

For example, Liska introduces the concepts of "parity" and "preeminence." What do they mean? Liska: "In its meaning of equivalence or equivalent standing, parity differs from both 'equality' and 'balance' " because it can sometimes imply "unequal restraints" and "unequal distribution" of assets (p. 21). Parity is then distinguished from preeminence: "As parity is to balance, so preeminence is to preponderance in that it connotes overall acknowledged standing. . ." (p. 22); and yet one finds that "primacy and parity" may be combined in "one notion": *"primus inter pares"*(p. 23). Thus armed with definitions of sorts, the reader is confronted with "parity issues," "parity mixes," and "parity agreements." Halfway through the book a further clarification by means of distinction is offered: "Parity connotes equal right or opportunity to different sources of power and forms in which power is manifest and employed. Preeminence connotes acknowledged superiority, based on privileged possession of critical assets and a related role" (p. 147). The reader struggles on. In the concluding chapter Liska reveals why these essences of meaning are so difficult to grasp. "Parity," he writes, "is, in the last analysis, along with several other key normative and strategic concepts (such as intervention and the balance of power itself) essentially and fruitfully a metaphysical notion" (p. 212).

Indeed, the *Quest for Equilibrium* is largely a metaphysical work, often in the perjorative sense. It is certainly abstruse. And it may be dealing with first principles, if equilibrium theory touches upon them. But the author's style makes it difficult to grasp his meaning at various points. Moreover, the author mars the text with such questionable words as "concretize" (p. 188), "navalize" (p. 197), "superadded" (p. 198), and "detraumatized" (p. 208). Proper editorial guidance would undoubtedly have improved this book considerably.

Despite its faults, editorial and substantive, *Quest for Equilibrium* effectively places the United States in historical perspective and illustrates imaginatively the major options that fact American statescraft in a post-Vietnam setting. Liska's treatment of America's ascendency, and of current alternative choices and their potential consequences, is truly unique. His analysis is complex and insightful. His attempt to draw consistently upon historical analogies as an analytical technique is impressive, but not always convincing. Historical parallels are more interesting as antecedents and lessons than as probable patterns of recurrence determined by systemic variables. But the combination of intuitive political sense and sensitivity to general historical parallels make this book worth reading.

DAVID W. TARR

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Model or Ally? The Communist Powers and the Developing Countries. By Richard Lowenthal. (New York: Oxford University Press, 1977. Pp. 400. $12.95.)

There is some justification for attempting to assess the foreign policies and international predispositions of the major powers, particularly as they relate to the modernizing interests, priorities, and efforts of a large number of twentieth-century nations. It would be helpful to see how the different perspectives of the major powers and those of much less important nations mix, and what results from such a blend.

This collection of essays by Richard Lowenthal sets out to forge such a linkage, in this instance between Soviet foreign policy, almost exclusively, and the various regime-building schemes adopted by modernizing nations. The essays span the period 1963–1976 (even the epilogue is a reprint), which was a fertile time indeed.

Lowenthal's primary concern is to expose what he finds to be a most disturbing problem: the Soviet Union, by offering a one-party control model of modernization, has caught the eye of many would-be and recently-emergent regime builders. Unfortunately, he carries this concern into all corners of his analysis.

The Soviets, according to him, want their modernization model to be accepted primarily because of what he views as far-flung Soviet international designs. He does suggest that there might be some attractiveness in a control model of regime modernization from the standpoint of the modernizer, but this is to him a secondary consideration, at best. His tunnel vision makes him focus on or at the least emphasize the presence of malevolent Soviet motives rather than modernizing issues and dilemmas. Lowenthal concentrates upon outlining, through a series of often superficially related examples, how the Soviets have attempted to bring ruin to otherwise honest attempts at political change.

One of the major pitfalls in the author's scheme of things surfaces during his numerous discussions of change situations. He does not put his observations into any kind of international political context and speaks to matters as if the only players on the board are the Soviet Union and the respective decision-making modernizer. There is an absence of any useful analysis of the internal and external environments within which regime-building decisions have been made.

What the author offers is a shopworn international muckraking effort that gives an awful lot of credit to the Soviet Union for "having its act together." If one is to believe Lowenthal, even for a moment, one has to say that the Soviet Union has done a remarkable job in forging a world more to their liking than otherwise. The author also postulates and assumes what he should indeed be trying to probe and assess. He should be probing the present two-way linkages between modernization efforts and the foreign policies of the Soviet Union and other communist nations, and should not be simply presuming them from weak casual evidence.

He does occasionally offer insights into some of the internal dilemmas within the Soviet foreign policy-making group as they hasten to react to problems and international changes, and sporadically, almost accidentally, he does mention some interesting points about modernizing regimes. However, in my estimation we should not take him too seriously even when he is insightful. He is not trying to analyze and educate but rather convince and persuade. These essays are not meant to be, or at least they do not appear as, political science but rather political messages.

In sum, this is not a good collection. There are occasional tidbits around, but they are well hidden and sparse. I do not recommend it except for those who are interested in studying a particular mind-set, although not necessarily a very unique one. It is a good primer for those interested in a cold war social democratic or liberal anticommunist posture but is devoid of useful political analysis.

CARL F. PINKELE

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Interdependence: The European-American Connection in the Global Context. By Gerhard Mally. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xviii + 229. $17.50.)

Gerhard Mally has provided us with a useful survey of the problems arising from the sharp increase in interdependence among the various actors in international relations. He is quite correct in pointing out that such interdependence is not always a source of greater cooperation, since it can also be a source of conflict. In fact, by itself interdependence is neither good nor bad, but has a potential for both, depending upon how the various actors perceive it. He also rightly notes that increased mutual dependence is seldom even; rather, such interdependence is most often uneven and results in a new form of dependence on the part of the weaker party in this uneasy symbiotic relationship.

In a brief opening chapter, Mally dissects the general concept of interdependence by breaking it down into various abstract categories or typologies. Such a taxonomy is not very helpful unless each category of thought is applied in some detail to a given set of interdependencies, with the result that the general theoretical propositions take on substance. But after stating the different possible types of interdependence in a brief categorial manner, he does not seek to relate these definitions to his subsequent substantive discussions of actual instances of interdependence. In the course of the latter, Mally surveys interdependence as it arises from considerations of security policy, problems of global ecology, the world economy and the political system of national states. The last parts of the book deal with the problems of managing interdependence in the Atlantic Community, where such interdependence has reached a higher level of interaction, as compared to other regions of the world. Mally particularly concentrates upon security and economic issues among the industrial democracies.

A more subtle and successful theoretical study of the problems of interdependence has appeared recently in the book by Robert O. Keohane and Joseph S. Nye, *Power and Interdependence: World Politics in Transition* (Boston: Little, Brown, 1977). As an explanation of this intriguing question, this work goes farther to satisfy the reader than Mally's book, which appears somewhat superficial by comparison.

Furthermore, there were a number of particular issues in Mally's discussion of "Interdependence in the West" which are subject to criticism. The North Atlantic Council, he holds, "arrives at decisions by unanimous vote" (p. 49). But as former U.S. Ambassador to NATO, Harlan Cleveland reminds us, the North Atlantic Council "never takes a vote, and seldom passes a resolution." Instead it operates by consensus something like a Quaker meeting. Further as former secretaries-general Stikker and Brosio point out, among others, NATO has taken action on many occasions by means of partial agreement. (See Elliot R. Goodman, *The Fate of the Atlantic Community* [New York: Praeger, 1975], pp. 343–45).

Mally also names eight countries as members of the Nuclear Planning group (p. 49). Actually there are eleven (he omits Denmark, Turkey and Norway), but with four permanent and seven rotating members, the active membership at any one times varies between seven and eight. In addition, on page 49 Mally neglects to mention that Greece, as well as France, does not participate in the Defense Planning Committee of NATO. He further mistakenly says that the doctrine of flexible response "lowers the nuclear threshold" (p. 50). The aim of this doctrine was precisely the opposite: to raise this threshold. Mally also refers vaguely to a "new Arms Procurement Agency" presumably created by NATO in late 1975 (p. 53). I presume he is referring to the European Program Group, which included France, and which therefore was created outside NATO.

In his section on "Conflicting Designs for the European-American Connection" Mally commits other errors. He says that the most prominent American official to support the concept of Atlantic federalism was Nelson Rockefeller, and that in considering Atlantic Union resolutions in Congress the executive branch had always opposed the concept of Atlantic federation (pp. 112–13). Wrong on both counts. Richard Nixon expressed approval of this idea on various occasions (though with vacillations), and during Nixon's presidency, in 1972, the State Department explicitly withdrew its opposition to Atlantic Union proposals in Congress. (See Goodman, pp. 28–29, 41–43.)

Mally then goes on to perpetrate another misunderstanding about the Atlantic Union proposal. The Europeans, he says, "are unwilling to institutionalize American hegemony in an organization, which–allegedly–relegates them to an inferior status" (p. 115). This indeed, is how the problem is often wrongly perceived. As Streit points out, the combined population of the non-American states in an Atlantic federation would exceed that of the United States, thereby placing the United States in a minority position. The Europeans who have taken the trouble to consider the Atlantic Union idea either have not understood this, or perhaps understanding it, have still rejected it as lacking in credibility. (See Goodman, pp. 14–19.) In any event, Mally too simply disposes of a rather complicated idea.

Finally, it was bad luck that this book went to press at a time when it appeared that the Communist party might take control of the government in Portugal. Instead of worrying about the impact of this eventuality of the Western Alliance (p. 54), it would be more appropriate today to worry about the consequences of Communists obtaining ministerial posts in Italy and France.

ELLIOT R. GOODMAN

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The Management of Defence. Edited by Laurence Martin. (New York: St. Martin's, 1976. Pp. xvi + 137. $15.95.)

This anthology presents six papers given initially at the National Defence College of the United Kingdom in late 1974 together with an introductory essay by the editor. With one exception, the authors are British (academics and current or former defense officials). The essays are thoughtful and uniformly unpretentious as they set about assessing the implications of changes primarily in the management of the British defense establishment, involving the increased use of by-now-familiar approaches from applied economics and operations research. Accordingly, they provide a healthy antidote to a tendency among U.S. experts to attribute too much in the way of both power and illusion to three orientations toward the defense policy process–unitary rationality, bureaucratic politics, and interstate systemics.

Explicitly and implicitly the authors place decisions about the allocation of defense budgetary resources between hardware alternatives in a context of other decisions on such matters as national security goals and priorities, where and how force will be used, the logistical evidence that the hardware will perform as its designers contend it will, manpower policy as it bears on costs and competence, and coordination across the military arms of a particular nation and between allies. Accordingly, they steer clear of two fallacies often found in discussions of defense policy processes. The first is the fallacy that rigor and rationality about one subset of decisions suffice to make defense policy as a whole rational. The second is the fallacy that, in the absence of comprehensive rationality, clarity and order in a particular subset of decisions is meaningless. A particularly strong point of the volume is the separation between rationality in the comparative cost-effectiveness of alternatives and rationality in the choice of the defense policy alternative with the greatest expected value.

With occasional bows to war-gaming, the volume seems stronger at supporting moderation in the claims made for the toolkit from applied economics than in illuminating ways to link such analyses to more rigorous treatments of the other decisions involved in defense management. Other than noting the importance of exploring alternative assumptions through game-type exercises, the authors seem to have little confidence in the value of analytic innovations for the integrative problems which policy presents. We are back to "wisdom" in spite of rather compelling demonstrations that we should not feel sanguine about the sufficiency of wisdom in defense policy processes.

One may admire the authors' calm in such a situation while not sharing it. Surely sufficient time has now passed since the innovations associated in the U.S. with the McNamara era for certain other changes in defense management to seem compelling. Some have to do with contextually-based comparisons of the cost-effectiveness of alternative weapons systems. Examples would include sensitivity analyses using the standard operations research tools to discover the implications of variables such as: (a) the creative or alternative adversary; (b) cost overruns which reduce the size of the force eventually procured; (c) delays in the development and procurement process which alter the strategic and tactical world in which the hardware in question will operate; (d) changes in national doctrine and strategy which alter the way in which political authority will exercise force, whether for deterrence or war or the gray situations in between these extremes. These considerations matter if one wishes defense managers and those who mandate them to reach better judgments about the national security utility of alternative resource allocations. Surely by now reasonably well-known techniques of developing and sampling alternative futures through decision trees, net assessment of competing defense forces, and dynamic man-machine simulations can be of modest help.

If, on the other hand, the management techniques in this volume are matters of implementation and instrumentation, then there seems to be a still unmet neet for what one may call political accountancy and comptrollership in contrast to political economy. Quite simply, the implementation of the alternative most attractive in the analyses should be compatible with the desiderata which led it to seem most attractive when the analysis was conducted. Warning needs to be given when these desiderata are in danger of becoming inapplicable. The resource allocation process needs to be opened up once again rather than allowed to potter along following its previously selected course. Neither the British nor the U.S. defense management organizations have shown themselves to be particularly acute at acting "rationally" on such warning or indeed seeking them actively.

It is important to note that the defense management, at least as it involves explicit analytics, with which the authors deal resides in defense organizations. Here it is unfortunate that the possible contribution of these tools to broader issues of national resource allocation between defense and other sectors of government goes unaddressed as does the value of applied economic analyses to higher levels trying to review independently judgments and recommendations from defense organizations Perhaps it is for this reason, or perhaps because of more general constraints on British defense expenditure, that we find little discussion of two truly fascinating questions for comparative analyses of defense decision processes. How are decisions made about how much is enough and with what consequences? And, how and when are decisions made that one cannot afford enough of something to make any expenditure worthwhile?

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Nuclear Proliferation and the Near-Nuclear Countries. Edited by Onkar Marwah and Ann Schulz. (Cambridge, Mass.: Ballinger, J.B. Lippincott, 1975. Pp. xv + 350. $16.50.)

Two tendencies are apparent in recent literature about nuclear proliferation. The first is to regard proliferation as inevitable, and to focus on learning to live with its consequences rather than preventing it. The second, related to the first, is increased respect for the particular interests that impel potential nuclear-weapon states to "go nuclear," and increased focus on individual cases rather than general patterns (there being no "typical" proliferator). The Marwah and Schulz volume represents these tendencies, as does the earlier book of readings edited by Robert M. Lawrence and Joel Larus (*Nuclear Proliferation: Phase II*, Lawrence: University Press of Kansas, 1974). By their nature, both books are uneven, but Marwah's and Schulz's book is probably more useful by virtue of its more complete coverage and in being somewhat more up-to-date in a rapidly evolving field.

The papers offered here were originally given at a conference held at Clark University in anticipation of the Non-Proliferation Treaty (NPT) Review Conference in 1975. The editors' introduction and five of the articles cover general aspects of proliferation; the remaining seven chapters are case studies of "near-nuclear" countries. Conference discussions were condensed into three short discussion essays, which were unfortunately too truncated to be very helpful. An epilogue by Marwah adds an insightful account of the NPT Review Conference and the reasons for its singular lack of success.

In general, the articles stress the recognized factors which make future proliferation more likely. First, the technical obstacles to proliferation are crumbling as the spread of reprocessing technology and entire fuel cycles legitimizes "near-nuclear" status (and the Indian ruse of "peaceful nuclear explosives" bridges the final gap between "military" and "nonmilitary" uses of the atom). Secondly, as technical limits recede, external and internal political-strategic incentives become decisive in nuclear decisions (the innovation here is the stress on *internal* pro-proliferation forces). Third, there is the ever-stronger conviction, which the Indian case apparently reinforced, that "nuclear power and international power are intimately related" (p. 261).

Finally and most importantly, these papers document in case after case the legitimate grievances of non-nuclear states regarding the inequality embodied in the NPT regime, and the failure of the superpowers to redress this inequality or break the link between nuclear weapons and international status. Stefan Leader and Barry Schneider underline the negative impact of failure to control vertical proliferation (the U.S.-USSR arms race) on efforts to prevent horizontal proliferation, while incidentally providing a succinct measurement of the strategic weapons balance. Robert Lawrence describes how superpower behavior has contributed to the decisions of three Indian Ocean states *not* to sign the NPT, and in the Indian case study K. Subramanyam—Director of the Indian Institute for Defence Studies and Analysis—attacks the superpowers for their attempted cartelization of nuclear weapons in language that may reflect particular Indian sensitivities but which should be read as an expression of widely held attitudes. As the editors remark (p. 15), there is "need for construing disarmament procedures in frameworks of equal validity to great and second order states alike."

In the course of thus recasting the debate, Saul Cohen offers some suggestive observations on "The Emergence of a New Second Order of Powers in the International System." Both Cohen, and Steven Rosen in his case study of Israel, suggest that nuclearization might actually stabilize local conflict situations; Rosen's argument rests on a proof of the feasibility of "second-strike capability" in the Middle East that is more persuasive in its technological aspects than it is politically or psychologically. The "post-proliferation" topics also include Christoph Hohenemser's preliminary look at the safety problems of nuclear fuel cycles and Theodore Taylor's restatement of his well-known concerns regarding theft and illicit diversion of nuclear materials.

At least five of the case studies—those concerning India, Israel, Iran, Brazil, and Argentina—tend to accept the inevitability of proliferation. The dynamics of local conflicts and competition is a decisive argument in Rosen's study of Israel, and to some extent in the South American cases (H. Jon Rosenbaum on Brazil, C. H. Waisman on Argentina). Brazil and Argentina are also striking cases of the influence of internal politics, though neither author judges such motives for proliferation to be decisive. South America appears here as a more likely arena for proliferation than is generally appreciated, though Rosenbaum feels the "peaceful nuclear explosive" route is most likely for Brazil while Waisman suggests that the "Israeli option" of calculated ambiguity may fit both countries. Anne Hessing Cahn's incisive analysis of the Iranian case concludes that a nuclearized Iran is likely by the end of the century, demonstrating among other things how little significance can actually be attached to ratification of the NPT.

Edouard Bustin's analysis of the South African case puts great stress on the general foreign policy context, and is therefore sensitive to the costs of a "public" bomb and other considerations that, again as in the Israeli case, make a near-nuclear ambiguity (rather than open nuclear weapons status) attractive in the short term. But the only hopeful case study from an anti-proliferation viewpoint is the chapter on Japan by Yoshiyasu Sato, a Japanese diplomat. Sato, however, takes an advocate's position against a Japanese bomb and overstates the case; not only does he exaggerate the political costs of Japanese nuclearization, but also he even claims technical deficiencies and—unlikely as it sounds—lack of adequate finances. A useful corrective to Sato's analysis is the pseudonymous article by a Japanese scholar in the Lawrence and Larus volume.

Despite these gloomy projections, however, the evidence presented does not justify total surrender to the inevitability argument. In the first place, the new stress on the particular motivations of each potential proliferator, case-by-case, should lead to de-emphasizing fears of explosive, chain-reaction proliferation. Secondly, the demonstrated and understandable unwillingness of non-nuclear states to forswear the nuclear option permanently does not mean that immediate pursuit of full and open nuclear weapons status—the other extreme—is inevitable. It may be unrealistic for heavily nuclearized powers to try to force second-order states to take a public pledge of abstinence (the NPT approach), but it might not be unreasonable to pursue the strategy of inducing these states to remain in the various near-nuclear or ambiguous postures to which many seem to be gravitating. Furthermore, as most of these studies indicate, at least implicitly, decisions in these matters will still be made largely in response to political-strategic and psychological (prestige) calculations that can still be influenced greatly by the existing nuclear powers—if they finally begin to take seriously the attacks on the inequities of the NPT approach and demands for limits to vertical proliferation.

ALAN DOWTY

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American Geopolitics and India. By Baldev Raj Nayar. (Columbia, Mo.: South Asia Books, 1976. Pp. viii + 246. $12.00.)

Baldev Raj Nayar is a competent scholar. He has written four books of superior quality on India: *Minority Politics in the Punjab* (1966), *National Communication and Language Policy in India* (1969), *The Modernization Imperative and Indian Planning* (1972), and *Violence and Crime in India: A Quantitative Study* (1975). Unfortunately, in his new book he departs from his areas of competence and from the canons of scholarship. He has written a long and repetitive tirade against the United States, based mainly on the writings of revisionist historians, selective quotations from statements made in congressional hearings, and his own biases.

His main charges against the United States are repeated like a cracked record. The "policy thrust" of the United States is toward "converting the entire globe into an American sphere of influence" (p. 68). It "has pursued the twin policies of satellization and containment of middle powers" (p. 5). It "has been opposed to the rise of independent centers of power, regardless of the nature of their internal political institutions" (p. 228). It "helps only satellites and that too only for its own purposes" (p. 79).

This basic geopolitical approach makes the United States a security threat to India. The "American rhetoric" is couched in terms of friendship and cooperation, but the reality is to be found in "the unilateral imposition by the U.S. of a threatening strategic environment on India" (p. 231). "The single dominant purpose of the U.S. in relation to South Asia is military containment of India" (p. 214). "A strong and powerful India is unacceptable to the United States" (p. 156). The United States seeks "a position of dominance" in South Asia. In fact it, and not India, is "the paramount power, the dominant power, or the preeminent power in the subcontinent" (p. 167). There is now an "American protectorate over South Asia," and it is assuming ever more insidious forms. "In contrast to the earlier situation, where the American policy of containment operated primarily as a U.S.-Pakistan alliance, it is now implemented through a grand coalition consisting of the U.S., China, Pakistan, Iran and Saudi Arabia" (p. 149). India and the United States, in short, are locked into a "fundamental strategic conflict."

And so on, and so on, page after page. Nayar states his central theme at the beginning of his book, and he sticks to it to the end. His selective approach is so unbalanced that it is hard to regard it as a serious analysis of America's foreign policies or of Indo-American relations.

Toward the end of the book Nayar reveals what seems to be the real reason for his apprehensions. India and the United States are condemned to "essentially adversary relationships" because India is "a middle power" and the United States is "a global power." "The fact that the United States is the mightiest power on the globe poses profoundly disturbing life-and-death choices for India" (p. 226).

Nayar's book is simply an expansion of themes which he has already stated in various articles, including one in an Indian journal, *Seminar,* and another in an American magazine, *Foreign Policy.* His book adds little to his magazine pieces. It lacks depth and substance, and it makes no theoretical or conceptual contribution. It should be taken seriously, however, as he argued India should be taken in his *Foreign Policy* article, for it reflects the views of many people in India and other countries of the Third World, as well as in Communist countries. Fortunately, there are many in India and the United States who have a different assessment of inescapable geopolitical realities and who believe that the larger interests of both countries call for cooperative relationships rather than "strategic conflict."

NORMAN D. PALMER

University of Pennsylvania

International Crisis: The Outbreak of World War I. By Eugenia V. Nomikos and Robert C. North. (Montreal and London: McGill-Queen's University Press, 1976. Pp. xv + 339. $23.00.)

This book is one of numerous studies on the origins of World War I undertaken by Robert North and various associates in terms of crisis management theory. A companion volume by North and Nazli Choucri, *Nations in Conflict: Domestic Growth and International Violence* (San Francisco: Freeman, 1975) analyzes the long-range dynamics of pre-1914 international competition. This work's theme is "the concept of crisis and the dynamics of the 1914 escalation" (p. 7). Three short introductory chapters establish the problem, describe the setting for the crisis, and offer general views on crisis decision making. There follows a day-by-day narrative of the events of June 28–August 12, 1914, purporting to demonstrate that the war resulted from misperceptions, miscalculations, and general mismanagement under crisis conditions. Three concluding chapters summarize the argument and propose certain contributions to crisis theory and prescriptions for crisis management.

The book can be criticized on various grounds. The diplomatic narrative, though fairly clear and accurate, is also choppy (53 chapters in 180 pages), superficial (consisting largely of summaries of messages and actions), burdened with some irrelevant detail, and above all excruciatingly familiar. Theoretical points seldom rise above the level of truisms, and the exposition is not entirely free of jargon (e.g., pp. 20–21) and of surprise at the obvious. Little of the massive and important literature on the origins of the war written since 1945 is represented in the bibliography.

But to dismiss the book on these grounds would be unfair and miss the point. Its purpose is not historical explanation, but analysis of a crisis in the interest of crisis management theory—a legitimate aim. The main question therefore is, does it work?

It depends on whether 1914 represents a crisis of the proper kind—one in which crisis management was crucial because a better understanding of the dynamics of crisis could have led to better decision making and the avoidance of war. The authors offer apparently plausible arguments that this was the case. Intensified international competition had made crisis and conflict more likely by 1914, they recognize, but had not rendered war inevitable. State behavior is not automatic or beyond human control; it can still be interpreted on a modified personal-behavior model. The decisions political leaders make in a crisis are genuine choices between alternatives, and these have great consequences. Why not then concentrate upon the dynamics of crisis and the understanding and influencing of decision making?

The reason is that 1914 was only superficially the kind of crisis the authors take it to be: the result of a particular incident which, occurring in an explosive atmosphere, escalated into war. More profoundly, the crisis derived from the fact that by 1914 international competition had reached a critical stage, in which a decisive outcome now appeared likely, even imminent, to the various powers—an outcome favorable to one side and unfavorable, even disastrous, to the other. The basic crisis therefore concerned the fundamentally irreconcilable resolves of the two sides in the face of his likely outcome: the determination of Austria-Hungary and Germany to stop and reverse the existing trend even at the risk of war, and the equal determination of Serbia, Russia, France, and (reluctantly) Britain not to allow it to be reversed. The authors frequently explain how the leaders of 1914 misperceived reality, miscalculated on the results of their actions, failed to get beyond their own narrow view. In fact, these leaders' perceptions of the basic realities were sounder and more profound than the authors', and were largely a matter of consensus. Austrian statesmen saw that Austria was losing disastrously, and that unless it drastically reversed existing trends she would soon cease to be a great power and might well disintegrate from internal and external pressures. Almost everyone agreed with this diagnosis, friend and foe alike. The problem was that only Germany, after long hesitation, agreed that Austria must do something drastic to alter this basic reality. When the authors condemn Austria for ignoring various alternatives for action in its blind determination to punish Serbia, they miss the point entirely. Of course Austria had choices *if* all it wished to do was to punish Serbia just as great powers generally try to teach obstreperous small neighbors a lesson. But this was not at all what it wanted. Austria wanted to eliminate Serbia as a political factor in the Balkans, in order to reverse the whole trend of Balkan and European politics. Having such a goal, Austria had no choices, no means available to make this tolerable either to Serbia or Russia.

Much the same thing is true of the wider struggle. The Germans foresaw that with Austria weakened, by 1917 Russia and France might become too strong for them, an intolerable prospect. The French and Russians saw exactly the same prospect and were determined to bring it about. And when the authors assert (p. 246) that "there was virtually no obstacle to an understanding or arrangement between Germany and Britain other than the anxieties, suspicions, fears, and ambitions of German and British leaders," they need first to examine the massive research extant on Anglo-German rivalry, and then to explain how either Germany or Britain could ever be brought to accept the outcome pursued by the other, namely, that the other power's empire, trade, and world position should be dependent upon one's own good will. For that, finally, was what Anglo-German rivalry was about.

Admittedly, many crises are solved by crisis management; the suggestions given here, obvious and anodyne though they seem, are often useful. And, since one cannot assign arbitrary limits to life and history, it is even barely possible that better crisis management could have postponed World War I. It never hurts to try. But anyone interested in crisis management theory should pay at least as much attention to its severe inherent limitations as to its possibilities. While even intense international competition can be maintained without war so long as the final outcome seems in doubt, once that final outcome, considered intolerable by one side and vital to the other, appears clearly foreshadowed, a resort to violence to reverse that outcome or to maintain and confirm it becomes extremely likely. This was the case in 1914, and most past wars and present threats of war (the Mideast!) are in this respect like 1914. It is therefore not enough to concede as the authors do that techniques of crisis management will work only if all the major parties prefer peace. They will work only if all parties, preferring peace, also somehow handle the problem of unacceptable outcomes. The prospective loser must come to terms with an intolerable loss, and/or the prospective winner must forego a vital gain, and/or someone, somehow, must assure that there is to be no decisive outcome at all.

PAUL W. SCHROEDER

University of Illinois

To Keep the Peace: The United Nations Condemnatory Resolution. By William W. Orbach. (Lexington: University Press of Kentucky, 1977. Pp. ix + 155. $13.75.)

This modest book brings together some useful material on the subject of condemnatory resolutions. It contains little, however, that will surprise scholars of international organization. The author provides a short essay on "resolutions of collective deligitimization," after which he offers comprehensive case studies of three such measures: a 1951 Security Council condemnation of Chinese "aggression" in Korea, a 1962 General Assembly condemnation of South Africa, and a 1968 Security Council condemnation of an Israeli retaliatory raid into Jordan. The 1951 resolution reflected cold war attitudes when the UN was still dominated by its western members; the latter two resolutions were introduced after the newer third-world countries became more influential. Orbach rightly sees each resolution revealing different aspects of the condemnation process.

The author occasionally displays real insight. His analysis of the convergence of political and moral factors behind most condemnatory resolutions is especially good. So too is his closing chapter about the efficacy of these resolutions where he concludes that the UN should focus on peace-keeping functions rather than on the politically inspired condemnatory measures. The latter, he feels, will eventually undermine the legitimacy of the UN itself.

The narrow scope of Orbach's subject will limit the book's appeal to international affairs specialists. Unfortunately, much of the book merely confirms what this audience already knows. Few students of international organization will be surprised to learn that the history of condemnatory resolutions reveals the UN to be a political body reflecting its members' interests, or to discover that the impact of these resolutions rests as much on their sources of support as on the size of their numerical majorities. Surely most scholars already realize that these resolutions usually are substitutes for more risky sanctions rather than preludes to such sanctions. And Orbach confirms our impressions when he demonstrates that condemnatory resolutions have become more common in tandem with growing third-world influence within the UN.

The author could have been more informative on certain other issues. It was the League of Nations, not the UN, which initially formalized the standards of international legitimacy. Orbach would have better illuminated the subject of legitimacy (with which he begins his book) had he briefly surveyed the League background.

The clash of assumptions between supporters and skeptics of post-1960 condemnatory resolutions also needs further examination. The frequently revolutionary assumptions of the supporters fundamentally challenge the legalistic assumptions of the western skeptics. To investigate this subject more fully would deepen our understanding not only of condemnatory resolutions, but of the broader dilemmas facing the contemporary UN.

Throughout the book, Orbach remains ambivalent about the utility of condemnatory resolutions. Here too a more sharply focused discussion would have been welcome. And on one very provocative point—Orbach's contention that the General Assembly effectively violates the UN Charter when it condemns a state for ignoring previous UN resolutions—the author is very murky. Orbach appears to be a strict constructionist on this matter, a defensible though challengable position. Whatever his view, the issue needs clarification.

Finally, two minor criticisms: *To Keep the Peace* could use a bibliography. It could also use a more descriptive title, for if Orbach proves anything, it is that the condemnatory resolutions have little to do with keeping the peace.

GARY B. OSTROWER

Alfred University

Harry Truman's China Policy: McCarthyism and the Diplomacy of Hysteria, 1947–1951. By Lewis McCarroll Purifoy. (New York: New Viewpoints, 1976. Pp. xv + 316. $6.95, paper.)

The thesis of *Harry Truman's China Policy* is that "it was the frenzied McCarthyite attacks upon the government for its alleged "softness on communism"–and only this–that brought about the reversal of America's policy of disengagement from the Asian continent and frightened policy-makers into a military-ideological crusade against Communist China" (p. xiv). Purifoy's preferred policy would have been to recognize that "the small states around the fringes of China (Korea, Formosa, Indochina, Burma and Thailand) . . . are, in large measure, racial and cultural extensions of China. Since this is China's natural 'sphere of influence,' it follows that any strong and independent government established in China will never rest until it has reestablished the same sort of dominance in the area that was once enjoyed by the Chinese Empire" (p. 148).

Purifoy argues that it was precisely such a policy of withdrawal from the continent to a Pacific defense chain running from the Aleutians through Japan, Okinawa and the Philippines, that the Truman administration had adopted in January, 1950, only to reverse course under the pressure of Senator Joseph McCarthy's attacks, beginning with his Wheeling, West Virginia, speech in February.

Others have argued convincingly that McCarthy's attacks exercised a major, continuing and pernicious influence on U.S. policy in Asia. Purifoy goes one step further and contends that this was the *only* factor influencing the reversal. In order to do this, he has to ignore or brush aside a great deal of evidence to the contrary, including the retrospective testimony of the key decision makers, and the connection between U.S. policies toward France and Indochina. *The Pentagon Papers,* for example, records that a week *before* Truman's announcement of U.S. disengagement from the Chinese civil war, and six weeks prior to McCarthy's Wheeling speech, the president had approved a National Security Council study calling for political, military and economic aid to resisting "further Communist expansion in Asia" (New York Times edition, New York: Bantam, 1971, p. 9).

But *The Pentagon Papers* is not cited in this book. Nor are Paige's *The Korean Decision* (New York: Free Press, 1968); Westerfield's *Foreign Policy and Party Politics: Pearl Harbor to Korea* (New Haven: Yale, 1955), which covers much of the same ground as Purifoy's study, more concisely and more persuasively; or any of a number of other standard works on the period. Though Purifoy makes frequent assertions concerning the nature of American public opinion, he gives only one footnote to the considerable volume of readily available published survey data, some of which seems unsupportive of Purifoy's contentions. Nor, apparently, did he use any of the basic source materials in the *Foreign Relations of the United States* volumes put out by the State Department, or the documents and oral history materials in the Truman Library.

Purifoy's tone is consistently polemic. The names of principal characters and institutions are frequently preceded by approbative or pejorative adjectives, to remind us who the "good guys" and "bad guys" are. Rather than presenting a single narrative, Purifoy moves back and forth in time from chapter to chapter, making it difficult to keep in mind the sequence of events, which is critical to his main hypothesis. There are a number of inconsistencies, also, in the book's treatment of some policy makers, arguments and events. Models of foreign policy decision makers put forward by political scientists (e.g., Allison's "bureaucratic polities" and "organizational process" paradigms) are not even mentioned in passing. Stylistically, the work is marred by the extensive use of such neologisms as "commie-bogieism," "containmentism," "ideologize," "Chiangian," "Asialationism," "conspiracists," and "salvationist." In addition, the index is not wholly reliable.

In short, political scientists seeking new findings on this critical period in the development of U.S.-China policy and in executive-congressional relations, or seeking a persuasive critical interpretation of its main events, will have to look elsewhere. Good starting points would be the Westerfield volume cited above, Tang Tsou's *America's Failure in China, 1941–1950* (Chicago: University of Chicago Press, 1963), and Ernest May, *The Truman Administration and China, 1945–1949* (Philadelphia: Lippincott, 1975).

SHELDON APPLETON

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Toward World Order and Human Dignity: Essays in Honor of Myres S. McDougal. Edited by W. Michael Reisman and Burns H. Weston. (New York: Free Press, 1976. Pp. xviii + 603. $20.00.)

As the title indicates, this volume is a collection of essays in honor of Myres S. McDougal, Sterling Professor of Law Emeritus at Yale University. As a prolific advocate of a policy-oriented and value-conscious jurisprudence, Myres S. McDougal is an important intellectual source for legal scholars and political scientists, especially those interested in international law. This is not to suggest that McDougal's jurisprudential approach has been unanimously or uncritically accepted and followed by his colleagues; however, no serious student of jurisprudence or international law can be uninformed about his work. A crude approximation of his influence is evident in the 15-page bibliography of McDougal's works, as well as books about him and influenced by him compiled by Frederick Samson Tipson.

Because of his policy orientation as well as his close collaboration with Harold D. Lasswell, who provides a brief introduction, political scientists will feel comfortable with and challenged by McDougal's ideas. Accordingly a brief statement of his intellectual thrust may be useful for those who are not familiar with his works.

McDougal offers a policy-oriented jurisprudence wherein the law must be conceptualized as a decision-making process that requires the articulation and clarification of values and the development of appropriate strategies to achieve desired outcomes. Consequently the law is not to be found in an immutable set of rules; it is to be found in the process of articulation and evaluation of claims for preferred values. The achievement of a world order devoted to human dignity, which he advocates, is also process-related in that it involves widespread and systematic individual participation in the shaping and sharing of values. The basic value categories that McDougal and his associates examine are those of power, wealth, enlightenment, skill, respect, rectitude, affection, and well-being.

As a collection, the individual essays are serious works of consistent high quality. With the exception of William T. Burke's essay, the analyses will not become dated. Burke's essay, "The International Law and Politics of Marine Science Research," was prepared before the opening of the UN Conference on the Law of the Sea in 1973. Although his essay is a very perceptive analysis of the problems of protecting marine science research and he accurately forecasts problems that have subsequently emerged at UNCLOS III, he has not commented upon the new developments.

The essays are tied together by a concern for and emphasis on either human dignity or world order. Although they share this common emphasis, the reader will find that the essays may be read independently in response to particular interests. Some of the essays have been presented previously.

A few of the essays deserve special mention. The first essay, "Myres S. McDougal and Twentieth-Century Jurisprudence: A Comparative Essay," by William L. Morison, is very useful in placing McDougal's emphasis within the context of his fellow legal scholars. As such, it is a valuable reference source. Readers who approach the essay with a generalist's rather than a specialist's interests and background may find Morison's ponderous style discomfiting.

Several other essays may have especial interest to political scientists. Burns H. Weston's essay, "The Role of Law in Promoting Peace and Violence: A Matter of Definition, Social Values, and Individual Responsibility," is a particularly provocative essay that integrates the concerns and perspectives of the lawyer and the social scientist.

The emphasis upon population policy, environmental deterioration, war, and human rights offered by Richard A. Falk in his essay, "The Role of Law in World Society: Present Crisis and Future Prospects," will be familiar to many readers. Readers interested in international law will applaud his assertion of the importance of that discipline in the study of world order. His call for "utopian engineering" (p. 160) to achieve that preferred world order will interest many readers.

Because of contemporary interest in human rights, Lung-chu Chen's essay, "Self-Determination as a Human Right," will be instructive to those concerned whether self-determination should be limited to colonial situations, as is the perspective in the United Nations, or whether it should enjoy universal application. From the perspective of support for human dignity, Chen argues for universal application of the principle.

John Norton Moore's essay, "The Legal Tradition and the Management of National Security," emphasizes the importance of international legal considerations in national security decisions notwithstanding the assertions of scholars who misperceive the utility of legal perspectives. He suggests several bureaucratic changes, such as the appointment of the State Department's Legal Advisor as a regular member of the National Security Council, to provide a continuous and systematic legal perspective.

Other essays included in the volume are: Rosalyn Higgins, "Integrations of Authority and Control: Trends in the Literature of International Law and International Relations"; James C. Miller, "Psychological Aspects of Systems Analysis"; Mary Ellen Caldwell, "Well-Being: Its Place Among Human Rights"; Egon Schwelb, "The Law of Treaties and Human Rights"; Gerhard Bebr, "Judicial Policy of the Court of Justice in Developing the Legal Order of the European Communities"; K. Venkata Raman, "Toward a General Theory of International Customary Law"; W. Michael Reisman and Eisuke Suzuki, "Recognition and Social Change in International Law: A Prologue for Decision-making"; and Douglas M. Johnston, "Facts and Value in the Prevention and Control of Marine Pollution." Eugene V. Rostow provides an afterword that offers instructive insights into the nature and character of myres McDougal as a person.

In its entirety, the volume represents a contribution to the study of world order and an appropriate tribute to a distinguished scholar.

DON C. PIPER

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Soviet Scholars and Soviet Foreign Policy: A Case Study in Soviet Policy towards India. By Richard B. Remnek. (Durham, N.C.: Carolina Academic Press, 1975. Pp. xiv + 343. $11.00.)

This is a useful study of the development of Soviet scholarship on India. Remnek seeks both to update works on Soviet policy toward India (including those of Arthur Stein, Robert Donaldson, and myself) and to analyze the role of Soviet academic orientalists in the policy-making process.

The major strength of the book is its thorough consideration of Soviet Indology and the excellent organization of that material in relation to themes and issues. Patterns are identified with regard to the ages, academic disciplines, and political orientations of scholars. The analysis of this literature is linked to a succinct and useful presentation of the changing directions of official Soviet policy towards India and South Asia.

Unfortunately, Remnek falls short in his effort to determine the degree and kind of influence wielded by Soviet academicians. Valid estimates of scholarly influence may well be beyond the capabilities of any western writer due to the lack of adequate information about formal and informal relations between scholars and official policy makers. Remnek makes little effort to apply to this task techniques and approaches of contemporary social science. Few official policy makers are even introduced. We need to know much about the relations among academic, party, and governmental personnel and structures. Do orientalists in each of the three sectors attend conferences or contribute to publications sponsored by others? Who competes with academics for the attention of official policy makers? What alternatives do official policy makers have to the research and perspectives of various academic specialists? Inferences regarding some of these questions might be drawn through analysis of personnel movements among the three groups, more rigorous comparative content analysis of academic and official pronouncements, and interviewing of Soviet scholars. Instead, Remnek primarily depends on occasional Soviet comments in publications and at major forums to develop his conclusions on scholarly influence. The result is a set of sweeping generalities that appear to underestimate the possibilities of such influence, especially during the eras of Stalin and Khrushchev.

Remnek asserts that under Stalin "one can hardly speak of any academic influence on Soviet state policy during this period" (p. 120). Such influence is deemed "imperceptible" at least through 1959. In Remnek's view, "If during the Stalin period Soviet orientalists played a supportive role in promoting revolutionary praxis, under Khrushchev they were not expected to elaborate retionalizations for the newly-established Soviet-Indian friendship" (p. 292). Real influence by Soviet scholars is viewed as a recent development supported by a more pragmatic and realistic post-Khrushchev Soviet regime. Academics are credited with helping to choose recent Soviet aid projects in India. Yet even this finding, based on scant evidence, is an isolated example of such influence.

Remnek notes that even the broad dimensions of Soviet policy have not been critically discussed in the academic literature and that "it would be presumptuous perhaps to expect any influence by Soviet scholars over the basic contours of Soviet policy towards India" (p. 293). Although I agree that open analysis of basic issues is rare, I am not persuaded that such evaluations are not provided through other channels.

Remnek's work remains a highly competent tour of Soviet Indology. However, it does not contribute very much to American Kremlinology except to provoke the questions that might lead to a more precise examination of possible relationships among Soviet academics and official policy makers.

RICHARD L. SIEGEL

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America as an Ordinary Country: U.S. Foreign Policy and the Future. Edited by Richard Rosecrance. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 226. $9.75.)

The Constitution and the Conduct of Foreign Policy. Edited by Francis O. Wilcox and Richard A. Frank. (New York: Praeger, 1976. Pp. xi + 138. $4.95, paper.)

In the aftermath of Vietnam and Watergate, analysts of American foreign policy have had to undertake their own agonizing reappraisals, first of the role of "imperial America" within the balance of power abroad and second, and more fundamentally, of the role of "the imperial presidency" within the separation of powers at home.

The first of these questions is the topic of *America as an Ordinary Country,* a book of essays edited by Richard Rosecrance. In his introduction, Rosecrance argues that America has become "an 'ordinary' or 'average' nation in that she cannot be expected to take on special responsibilities for world peace keeping, to use her military forces where others do not wish to become involved, to support the world economy and sustain an international financial structure organized around the dollar. Her role as maintainer of the system is at an end" (p. 11).

Rosecrance's own essays in the volume are perceptive, subtle, and judicious developments of this theme. They include insightful historical comparisons between America in the mid-twentieth century and Britain and Germany in the late nineteenth century. And they conclude with the prescription that, given the relative decline in American military and economic power and the increase in the number of other powerful actors, the U.S. should assume the role of balancer in the international system and should develop the skills of diplomacy in its foreign policy. Rosecrance's prescriptions for U.S. policies in the future have much in common, it seems, with Kissinger's justifications for his own policies in the past.

Three of the other essays in the same volume are among the best recent writings on their respective topics: Coral Bell on détente, Peter Katzenstein on the U.S. and West Germany, and Robert Gilpin on multinational corporations. Bell and Katzenstein are comprehensive, subtle, and wise in their analyses; more importantly, they are each able to say something new and interesting about very old and familiar subjects. Gilpin, in what is probably the best essay in the volume, develops some of the themes in his important and excellent book, *U.S. Power and the Multinational Corporation: The Political Economy of Direct Foreign Investment* (1975). One such theme is that American military power after 1945 led to American investment abroad which in turn has led to a decline in American economic power. Here, Gilpin also draws a comparison between mid-twentieth century America and late nineteenth-century Britain.

The remaining essays in the volume are all by well-known analysts of international affairs: the late Alastair Buchan on U.S. foreign policy in general, Pierre Hassner and Francois Duchêne each writing on the U.S. and Europe, Kenneth Hunt on the U.S. and the Far East, Leonard Silk on the U.S., in the world economy, and Fred Bergsten on the U.S. and the Third World. Each essay is a tour d'horizon of its topic and a snapshot of it as it existed around 1973–1976. They are sound and sober—and utterly conventional, ephemeral, and forgettable. That is, they have the virtues of good policy position papers, but a half-life of twelve months. It is no accident that these last six analysts have done most of their writing in policy institutes rather than in university departments. And in regard to the interests of professional political scientists, they have almost nothing significant to say.

Rosecrance's prescription that the U.S. enhance its diplomatic skills and pursue a balance-of-power policy abroad will be constrained, as Kissinger's experience repeatedly demonstrated, by the separation of powers at home. This raises the second question mentioned at the beginning of this review. The relationships between the president, the Congress, and the courts in the making of U.S. foreign policy are obviously important topics, and *The Constitution and the Conduct of Foreign Policy,* another book of essays edited by Francis O. Wilcox and Richard A. Frank, is an attempt to address it. The essays focus on the issues of secrecy in foreign policy, public participation in the foreign policy process, the constitutional power to make war, and the role of executive agreements in foreign policy.

Unfortunately, the result is not a success. The authors vary in their methods between simple descriptions of court decisions and pompous and vacuous prescriptions that there should be more cooperation between the president and Congress. There is no attempt to provide historical or empirical explanations of changes in the relationships between the three branches of government in the making of U.S. foreign policy, although one might have thought that sound prescriptions should be based upon sound diagnoses. The essays are written like judicial opinions; but while even a poorly reasoned and poorly written judicial opinion can wonderfully concentrate our minds by the prospect of the power of the state to enforce it, these pseudo-opinions possess no authority—political, intellectual, or stylistic—whatever. In general this volume is almost unless to professional political scientists—or to anyone else, for that matter. There are a couple of exceptions, however; George Reedy's short piece on secrecy shows the shrewdness and subtlety that is characteristic of his other writings on the presidency. And W. Taylor Reveley's essay includes an illuminating discussion of some contradictions in the 1973 War Powers Resolution.

Can a balance-of-power foreign policy coexist for long with a separation-of-powers constitutional system? These books do not really answer this question. The answer suggested by historical experience is rather negative. Neither Metternich, nor Bismarck, nor Disraeli, nor Churchill—the legendary heroes in the balance-of-power epic and the heroes of Henry Kissinger too—ever had to deal with the tedious checks and balances, the boring domesticity, of our Constitution. But even they had to deal continuously with their own variety of domestic politics, and each of them was one day driven from office and his balance-of-power policies dismantled and undone because of domestic pressures. In most political systems, it seems, and not just in that quaint antique designed by James Madison, balance-of-power politics must begin at home.

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OPEC: Success and Prospects. By Dankwart A. Rustow and John F. Mugno. (New York: New York University Press, 1976. Pp. iv + 172. $12.50.)

This book describes the creation and development of the Organization of Petroleum Exporting Countries (OPEC) and answers some common questions concerning the future of U.S.-OPEC relations. Implicit in the authors' purpose is the assumption (or concession) that America's economic future is ultimately and necessarily linked with that of the members of OPEC.

In 1961, the political leaders of some Middle East and Latin American states sought to gain economic control over their oil resources by forming OPEC, which did not gain momentum until the beginning of the 1970s. Why did OPEC succeed? Rustow and Mugno claim that the development of shared attitudes of "enlightened self-interest" on the part of these various oil-producing nations contains the answers. These shared values and concerns were best formulated in the Declaratory Statement of Principle in 1968. This document, which is based upon the concept of "changed circumstances," gives an outline of the process by which the resource-holding governments would gradually gain control of their petroleum reserves through reasonable participation in production and pricing. Distribution and exploration rights were not at issue at this time.

Historically, the principle of "changed circumstances" in international law had been the convenient means by which great powers relieved themselves of burdensome obligations toward smaller and less developed states. It was not, however, a principle which accorded similar privileges to small states desiring to relieve themselves of obligations incurred towards a larger power.

Recent transfers of control in OPEC to national governments have been proceeding without violent confrontation. Productive (albeit slow) bargaining has also been a notable characteristic of recent interactions between oil companies and OPEC governments. Rustow and Mugno assert that OPEC, as an economic organization, is stronger today than it has ever been and shows better prospects of continued success than had been previously projected.

The uniqueness of this short and well-written book lies in its insistence that OPEC is an economic organization and that "OPEC alone is putting to the test the time honored notion that economic forces are the determinants of history." Traditional theories of integration and cooperation such as those advanced two decades ago by K. Keutsch and others cannot explain how or why a ruler from Qatar or Libya can show much solidarity with another from Venezuela or Nigeria. None of the "political" indicators mentioned in the past have been adequate to explain OPEC's success. It is the economic imperative as pinpointed by Rustow and Mugno that makes the most sense in explaining the OPEC phenomenon. However, the question remains, if economic forces have helped OPEC during the past decade, will they continue to do so? The emphasis of the economic character of OPEC may be particularly troublesome in the context of the intermediate and contemporary stages of formation and consolidation of OPEC. Having established control over petroleum production and pricing, the problem now is that of creating stable and continuously rewarding relations among the OPEC member nations.

The authors' analysis of OPEC's growth and future prospects contains important detailed information about the circumstances leading to the formation of the producers' association and provides an interesting chronology of the early arrangements between the oil-producing companies and their national governments and the oil-resource countries. The authors assert that the acceptance of the doctrine of "changed circumstances" by the international companies (at least the U.S.-controlled ones) was largely a function of advice by the U.S. Department of State. This acceptance was made in the expectation that the price of oil would soon drop to its pre-OPEC control level. If the authors are correct, and I have no reason to doubt their assertion, some young doctoral candidate should certainly make this a dissertation topic!

Rustow and Mugno correctly observe that many analysts of OPEC and of the system of international economics have predicted an early demise of OPEC due to individual price cutting by OPEC members in order to produce more revenue from their oil sales. The subsequent failure of this prediction has puzzled those who dislike OPEC and who refuse to accept the notion that, in addition to being a powerful agent, OPEC has developed a strong political identity. The 1973 oil embargo placed upon the U.S. and others by the Arab members of OPEC is perhaps the outstanding example of the international political power wielded by this producers' association. The recent changes in the U.S. policy towards the Arab-Israeli conflict has, in fact, reflected OPEC's ability to bargain politically in the international arena. Furthermore, the impulse to break the association's price arrangements has probably been tempered by the desire among most members to maintain and/or actually increase the level of regional solidarity which OPEC represents. The obvious temptation of increasing oil revenues by increasing production has been in turn neutralized by organization-wide accepted price increases, resulting in revenue increases for most of the OPEC members (notably Iran and Saudi Arabia), and these have not ushered in OPEC's destruction. The authors insist that the continued need for economic redistribution in this area does suggest that OPEC will at least maintain, if not expand its role. Whether this happens or not, the authors' projections make a lot of sense indeed.

Finally, I am happy to see that Rustow and Mugno reject the shortsighted solution to the oil crisis, namely, the military one. They correctly argue that the "stability" of the *flow of oil* from OPEC to where it is used cannot be determined by simple-minded "gunboat" diplomacy. The cost of interrupting the oil flow to Europe, Japan and the U.S. would be prohibitive even if the initial attack were to "relatively succeed." Such an inter-nation interaction is doomed to ultimate failure. Rustow and Mugno's contention that international order is a function of future-oriented solutions is therefore entirely valid. Although the analysis used by the authors is admittedly a "traditional" one that relies on soft data and makes little use of measurement, on the whole I found *OPEC: Success and Prospects* a stimulating and balanced book.

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Henry Kissinger: The Anguish of Power. By John G. Stoessinger. (New York: W.W. Norton, 1976. Pp. x + 227. $8.95.)

Kissinger: The European Mind in American Policy. By Bruce Mazlish. (New York: Basic Books, 1976. Pp. vii + 298. $10.95.)

Henry Kissinger's impact upon American foreign policy and contemporary international politics is a subject that has preoccupied foreign affairs analysts since his arrival in Washington with the Nixon administration in 1969. More recently, the identification of the sources of his behavior is a task that has captured the attention of political scientists and psychohistorians. In these two books the authors attempt to link such concerns, and together they do provide a comprehensive treatment of Kissinger's personality traits, his political belief system, and his record as a foreign policy maker. However, Stoessinger, an international affairs analyst, tends to be more at home assessing the impact of Kissinger's actions, while psychohistorian Mazlish is more comfortable interpreting the sources of Kissinger's behavior.

In the Stoessinger volume only the first 40 pages are devoted to an analysis of Kissinger's beliefs and personality traits. The method of inquiry is a dialectical process in which Stoessinger identifies political beliefs in Kissinger's academic writings and then interprets them in the light of his own personal acquaintance with Kissinger, an association that began in graduate school at Harvard and has continued intermittently for a quarter of a century. Stoessinger supplements this analysis with ad hoc comments about Kissinger's personality traits and speculates about the interrelationships between these traits and Kissinger's youth as a Jew in Nazi Germany. The remainder of the book is a largely descriptive account of Kissinger's record as a policy maker and deals with Vietnam, the Middle East, China, Bangladesh, SALT and détente, Eurocommunism in Portugal and Italy, Angola, the Cyprus conflict, the food and energy crises, and racial strife in southern Africa. The only notable omissions appear to be the Chilean and Biafran conflicts. Stoessinger does intersperse his narrative with commentary that attempts to relate Kissinger's behavior to his political beliefs and occasionally criticizes the secretary of state's policies, especially regarding the Cyprus conflict and the development of Eurocommunism.

The emphasis in the Mazlish treatise of the Kissinger phenomenon is self-consciously psychoanalytical. Approximately half the book is concerned with the delineation of Kissinger's personality, an analysis based upon over 100 interviews with persons who knew him in Germany, at Harvard, and during his years in Washington. Another one-fifth of the volume is focused upon the identification of Kissinger's political beliefs, leaving only a few chapters for an analysis of Kissinger's behavior as a policy maker, and the latter are concerned largely with the Nixon-Kissinger relationship, Kissinger's orientation toward political power, and a critical assessment of his leadership capabilities.

Both Stoessinger and Mazlish agree that a knowledge of Kissinger's political beliefs is essential to understanding and assessing his political behavior. They also tend to identify some of the same themes as important elements of his belief system: a preoccupation with stability as the penultimate political goal, a high sensitivity to the possibilities for historical tragedy, a preference for negotiated solutions to international problems, and a recognition that a willingness to use force is necessary in order to survive in the international arena.

There are also differences in their interpretations of Kissinger's political beliefs. For example, Mazlish is unsure (p. 197) whether Kissinger believes that the USSR is no longer a revolutionary state, while Stoessinger (p. 43) maintains that Kissinger had decided as early as 1968 that both Russia and China had ceased to be revolutionary powers. This difference appears to be due to Stoessinger's reliance upon Kissinger's speechwriting activities for the Rockefeller presidential campaign as a basis for this judgment. Mazlish seems to be unaware of this source.

Such discrepancies are important, because of the emphasis that both authors place upon Kissinger's political beliefs in order to understand his policies. Even Mazlish, who is most interested in a psychoanalytical interpretation of Kissinger's political behavior, cautions that Kissinger's "ideology must be interpreted first in its own terms, and only then in more psychological terms.... As an adaptive mechanism, intellectual activity may be often subject to the constraint of extrapersonal interests and circumstances, to the virtual exclusion of intrapsychic dynamics" (p. 154). In contrast to President Nixon, who had "no conceptual system worthy of the name, that mediated between him and his psychological impulses," Kissinger's policies must be understood "in terms of his ratiocinations, which have taken on a life of their own. . ." (p. 285).

Mazlish nevertheless demonstrates some quite plausible connections between Kissinger's personality and certain features of his political world view. However, the author's judgment (p. 286) that Kissinger's world view has developed a consistency which is independent from fluctuations in his immediate personal needs forces Mazlish to conclude: "It is ultimately on the intellectual level, the level of Kissinger's conceptualization, that one must seek to evaluate him most comprehensively" (p. 291). This critical perspective is shared by Stoessinger, who recognizes that the strengths and weaknesses in Kissinger's record as a policy maker reflect the influence of various features in his belief system.

While Stoessinger tends to find fault with aspects of Kissinger's Vietnam policy, his handling of the Cyprus conflict, and his response to the issues raised by the Eurocommunist developments in Portugal and Italy, his overall judgment of Kissinger's performance is a sympathetic one. Mazlish's final assessment is less favorable than Stoessinger's. Kissinger's failure to integrate America's liberal domestic values with the elements of historical tragedy and realpolitik associated with his own "European" perspective, plus his faulty understanding of the "new world of revolutionary, political, social, and economic developments and aspirations . . . [makes him to Mazlish] . . . a hero too deeply flawed to serve as an inspiration for America in grappling with the awesome problems that face it. . ." (pp. 297–98).

STEPHEN G. WALKER

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Silk Glove Hegemony: Finnish-Soviet Relations, 1944–1974—A Case Study of the Theory of the Soft Sphere of Infuence. By John P. Vloyantes. (Kent, Ohio: Kent State University Press, 1975. Pp. xiii + 208. $10.00.)

Armed with a metaphor of the silk glove that sometimes bares a mailed fist, John Vloyantes explores the caprice and the capability of Soviet power in relation to Finland. The book ranks among the rare perspectives in English on Finno-Soviet relations since 1944. It offers a well-organized and useful summary of the major aspects of postwar Finnish foreign policy.

Vloyantes examines the development (1944–1948), dysfunction (1958–1962), and repair (1963–1968) of the Soviet system of influence in Finland. It is a system which he terms "soft sphere" ("a system of influence which is indirect and whose effects have due regard for the sovereignty of the sphere country," p. 22), as opposed to "hard sphere" ("the paramount power reduces the independence of sphere states to the condition of abject satellites or protectorates with one-sided concern for the interests of the superior powers," p. 21). Although the Soviets' soft-sphere policy toward Finland has suffered "lapses," Vloyantes maintains that after 1968 Moscow could be "more certain that the soft-sphere system would function as she desired" (p. 137). The author predicts that the soft sphere will remain "unimpaired for the foreseeable future" (p. 140).

In positing a continued soft approach to Finland by the Soviets, Vloyantes fails to give the evidence of the motivations he ascribes to the Soviet leadership. He contends, for example, that Soviet misgivings toward the Finnish Conservatives "may" recede because the party has "shown restraint" (p. 199). What in fact is the Soviet orientation toward that party? The Kremlin could conceivably ask that Conservative "restraint" be exchanged for a Conservative "fulfillment" similar to the change in policies elicited from the Social Democratic Party that Vloyantes calls the "Socialist fulfillment." Was 1968 the end of dysfunction in the Soviet soft-sphere system or was it the continuation of Soviet maneuvering in Finnish affairs?

Although the author readily admits the gap in Russian source material, the admission does not help remove this study from the realm of speculation regarding the familiar issues of Soviet intentions toward Finland and the nature and extent of the Kremlin connection in Finland. If instead of a Soviet perspective, the study is meant to focus on Finland's foreign policy, it lacks depth in its sparse use of Finnish sources. For example, inadequate attention is given the economic implications of the crises of 1958 and 1961, and Finland's participation in the European economic schemes. The overt changes in personnel in Finnish politics were accompanied by critical changes in the pattern of Finno-Soviet trade that research on Finland's foreign policy will in time reveal.

The use of soft and hard spheres as conceptual devices for analyzing influence raises questions as to the justification for adding new terminology to the existing catalog in influence relationships. The term "soft sphere" and "hard sphere" in Finno-Soviet relations might readily and more credibly have been replaced by "postwar Soviet policy toward Finland." An alternative to using simple language is to work with concepts as they have been used by others. Vloyantes does not draw in the literature on spheres of influence and influence in interstate relationships in general. How does the author's concept of influence differ, for example, from James Rosenau's penetrated political system? Wolfram Hanrieder's consensus and compatibility? Or the formulations on influence of J. David Singer? K. J. Holsti?

The failure to distinguish the soft-sphere concept in relation to the sources at hand is compounded by a more fundamental confusion—between assumption and hypothesis. The reader is left in doubt as to whether the author intends the soft sphere as a tool of analysis or a hypothesis to be proved. In the introductory framework for analysis, the properties of a soft-sphere relationship are outlined (pp. 1–32) and exemplified by the case of postwar Finno-Soviet relations. The plan of the study is suddenly interrupted, however, when the author asserts that "it is possible to conclude on the basis of this research that *there is* such a phenomenon as a soft sphere of influence" (p. 183). If the soft sphere is intended as a tool of analysis, what are the hypotheses? If it is intended as a hypothesis, is one case a sufficient demonstration of the hypothesis? In either circumstance, is a model relevant or necessary for the understanding of a single case? Does not the model tend to be simply a detailed description of the case rather than a generalized prescription of universal applicability?

If the author had focused on the Finno-Soviet relationship, unfettered by the model of the soft sphere, he might have presented a more incisive view of the critical issue: To what extent have the Soviets succeeded in conditioning the behavior of the Finns? Is there, in fact, a silk glove over the mailed fist or does the glove appear solely in the eyes of the one to whom it is extended? Have the Finns joined the benumbed crowd in which only the small innocent dares to exclaim, "But mother, the emperor has no clothes on at all!" Who has pulled the silk glove over Moscow's mailed fist—the Soviets, or the Finns?

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Policy-Making in the European Communities. Edited by Helen Wallace, William Wallace, and Carole Webb. (New York: John Wiley, 1977. Pp. 323. $21.95.)

This edited volume of case studies dealing with the policy-making process in the European Communities has been strongly influenced by

Theodore Lowi's observations on the relationship between case studies and cumulative theory building in political science. Thus, the various contributors are involved with the question of whether the Council of Ministers in Brussels has been able to shift the locus of Community policy making away from the control of individual member bureaucracies, and toward a larger supranational focus. The first and last chapters of the book begin and conclude on an unsure note. But there is little question that the plurality of viewpoints represented in this study does not obscure the basic argument that (a) the Community has become a hostage of the domestic political process within separate member states, and (b) that the movement toward a larger supranational purpose is, at best, a long way off. And finally, if the Community begins to move toward a sense of supranational purpose in the future, it will not be because the individual members have rationally decided to make the Community something more than it is. Rather, so the editors conclude, "If the Communities survive into the mid-1980s, and have in the meantime widened and strengthened their ability to formulate and implement policies in concert or in common, it is unlikely to have been the result of any overall strategy. Rather, we suggest, it is likely to be the cumulative outcome of a long series of reactive decisions, responding to domestic and international pressures in a piecemeal fashion" (p. 322).

The synthesis of case studies with theoretical constructs is ultimately determined by the ability of individual analysts to choose examples of "live" politics which can be cumulated into larger explanations. Case studies must be isomorphic in the first instance, theory relevant in the second instance. This volume comes off reasonably well on both counts, although the diversity of cases employed makes the effort to systematically relate singular instances with larger theoretical concerns an occationally tortuous one. Is the role of Brussels becoming increasingly dominant in the initial design and implementation of Community-wide projects, running the gamut from sugar, monetary, industrial and competition policy? No. But the individual cases answer the question in slightly different ways. In an extremely well-written and interesting selection dealing with fiscal harmonization efforts within the EEC, Donald Puchala suggests that at least in terms of West Germany and Italy, the introduction of a Value-Added Tax (VAT)–a goal supported by Brussels–is a function of domestic politics, not a response to the determinist logic behind European policy-making integration. In his words, then, "governments accept and implement 'European' policies to the extent that they serve national aims and interests" (p. 266).

Michael Hodges takes a slightly different path in determining how effective Brussels has been in the development of a common European industrial policy, but the ultimate answer is the same. Thus, "the failure to develop a comprehensive policy for industrial development at the Community level can be best explained by the absence of a consensus among member governments that such an approach would being them tangible benefits which would otherwise be unobtainable" (p. 130).

So where does this leave us? Certainly the various writers have made a strong case for the primacy of domestic over supranational politics; and the dominance of essentially "political" over "technical" considerations in the integration process is equally marked. But what about the future of the European Community vis-à-vis the role of Brussels as a focal point for policy initiation and implementation? Or, perhaps the more significant question is one of determining whether other forms of cooperation and policy-making between individual governments within the European Community need to be formulated? If national governments display purely instrumental attitudes toward future cooperation with Brussels, then perhaps the most effective means of achieving "some" form of European integration is via the intergovernmental route, and particularly in terms of systems which share interlocking economic, financial, and political interests with each other. Alternatively, if one is still favorable toward the traditional supranational approach to the study of European integration, with Brussels as the ultimate focus of European nation-building efforts, then perhaps new efforts have to be made in the direction of transforming routine technical and financial issues into "high" political meaning. Is there a way of re-emotionalizing the European question?

JOHN STARRELS

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Indonesian Foreign Policy and the Dilemma of Dependence: From Sukarno to Soeharto. By Franklin B. Weinstein. (Ithaca, N.Y.: Cornell University Press, 1976. Pp. 384. $17.50.)

Literature on Indonesian foreign policy has long been preoccupied with the domestic factors impinging on that policy, perhaps because of the comparative utility offered in the three markedly different regimes which have controlled the country since independence. In the same manner, this study has attempted to generalize on these domestic determinants by comparing Indonesian foreign policy under Guided Democracy (1958–1965) and the New Order (1966 to the present).

Examining the global perceptions of Indonesia's foreign policy elite through extensive interviews carried out during 1970 and 1973, Franklin Weinstein has identified a serious dilemma under which they operate. The elite views the world as hostile and exploitative, leaving them aware of the frailty of their independence and jealous for its future. Further, this vulnerability and underdevelopment can be eliminated only through massive mobilization of domestic resources or heavy reliance on foreign assistance.

Massive domestic mobilization has always appeared beyond the will of Indonesia's elite, but can sufficient foreign assistance be introduced without weakening Indonesia's independence? In an analysis reflective of Herbert Feith's *The Decline of Constitutional Democracy in Indonesia* (Cornell University Press, 1962), Weinstein described a constant political antagonism between amorphous groups that place priority on independence or aid. Weinstein has argued that competition within the political system tends to keep the government close to the independence stance. However, the incumbent leadership has typically taken a more favorable view of foreign assistance because of its need to provide visible achievements, while the opposition has found the threat of assistance a convenient rubric for attacking the government. From this, Weinstein developed a general hypothesis: "Where the foreign policy elite of an under-developed country perceives the world as hostile, intense political competition will lead the country toward a foreign policy that puts independence first, while a less competitive situation will permit a policy that accords priority to the search for aid" (p. 356).

In the "dilemma of dependence," Weinstein has identified an important fact of life in contemporary international politics. There are, however, several problems in his interpretation. First, the assistance-independence dichotomy appears misleading and biased. (Who, after all, would sell independence for a driveling of foreign aid or investment, particularly from the neocolonial West?) This does not lessen the dilemma, nor soften the insecurity in accepting assistance, but it does imply that the decision maker consciously places independence above assistance, albeit he may rather circuitously rationalize to do so. The dilemma arises precisely because the independence-assistance question is not dichotomous but rather is a gray spectrum. Also lost in the dichotomy is the obvious fact that selling one's independence is great cannon fodder for the opposition.

Historically, Weinstein argued that the elite's hostile view of the world took shape largely during the post-independence parliamentary period (1950–1957). But, there is ample evidence that the bloody revolutionary period was sufficient to instill this view. The distinction is important because the hypothesis breaks down in the parliamentary period, the only period in which broad and intense political competition was a reality in Indonesia. Sukarno subsequently restricted or eliminated significant elements of the political spectrum, a process further extended under Soeharto. Thus, each successive regime, according to the hypothesis, should have been more receptive to foreign assistance. Comparing the parliamentary and guided democracy regimes, it is obvious that the former was more favorably disposed to foreign assistance, despite the greater measure of political competition; hence, the necessity for a very dubious historical interpretation of that period.

Finally, the general methodological problems raised by the hypothesis are monumental. To effectively test the hypothesis would require that the psychological degree of hostility be causally linked to foreign policy outputs in correlation with increasing or decreasing domestic political competition. This point is not rhetorical because, as with most studies of the domestic determinants of foreign policy, Weinstein asks the reader to accept causal explanations for a foreign policy that is defined in only the most elemental terms. And consequently, while the study effectively presents some very interesting and useful insights into Indonesia's elite and vividly documents the not always latent feelings of insecurity and hostility within the Indonesian elite, it tells us very little about Indonesia's foreign policy.

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Crisis Management: Confrontation and Diplomacy in the Nuclear Age. By Phil Williams. (New York: Halsted Press, John Wiley, 1976. Pp. viii + 230. $16.95.)

In this book a British scholar invades an academic thicket that thus far has been a primary preserve of American students of national security in the Nuclear Age. Phil Williams conducts a professional examination of the concept of crisis management as developed in the academic literature and as practiced by the United States, the Soviet Union and to a lesser extent, Communist China during the cold war.

In the opening chapter Williams reviews the difficulties he encountered in what he described as a "relatively limited and straightforward task." The most important difficulty is the tremendous assymetry between public knowledge of the United States decision-making process compared to what goes on in the higher councils of the Soviet Union. Williams chose to focus on those crises relating to confrontations that "appeared to involve a distinct possibility that large-scale military hostilities might erupt between the superpowers." Such crises are important objects of study because they "provide the supreme challenge to the wisdom, resourcefulness and courage of the decision-makers involved."

Moving meticulously into his subject, Williams defines an international crisis as "a confrontation of two or more states, usually occupying a short time period, in which the probability of an outbreak of war between the participants is perceived to increase significantly" (p. 25). He then postulated two purposes of crisis management—to resolve confrontations peacefully and/or to gain advantage for one's national interest. As to this latter purpose, Williams concludes that "manipulating and influencing the opponent while simultaneously controlling events and avoiding war is a daunting task" (p. 30).

While asserting that crisis management, as thus understood, is perhaps unique to the nuclear age, the author proceeds to examine the confrontations leading up to the first and second World Wars. This period, however, is not over and done with. Immediately following his examination of the Cuban Missile Crisis as illustrating the "necessity of the measured response" we are again treated to the prelude to the guns of August, 1914. Williams' tendency to jump back and forth in crisis time and place occasionally seems disconcerting.

Williams introduces an interesting dialogue concerning the effect of the growth of Soviet strategic capabilities on both Soviet and American willingness to run risks during future superpower confrontations. In earlier confrontations with the United States, when the Soviets were bargaining from strategic weakness, it is alleged that they were far more cautious and restrained in their behavior than would have been necessary had the strategic balance been more favorable. Brzezinski suggested that in the 1962 Cuban Missile Crisis acute awareness of the inadequacies of the Soviet nuclear posture not only made Khrushchev withdraw Soviet missiles from Cuba but, as Brzezinski put it, also precluded possible countermoves elsewhere. "Had strategic symmetry prevailed it might have proven much more difficult for the United States to achieve its principal objective in Cuba [the removal of hostile missiles]" (p. 157).

After reviewing a number of crises from Quemoy (1958—U.S.-PRC), Berlin 1961, to the Yom Kippur War of 1973 during all of which the value of symbolic communication was stressed, Williams closed his penultimate chapter with these questions: "On what basis have the 'conventions' or 'rules' of crisis management been founded? Have the superpowers become increasingly adept in the art of crisis management as their relationship has developed and matured? Are the Soviet Union and the United States nevertheless moving towards the replacement of crisis management by a system of crisis prevention?"

Williams seems to believe that the two most powerful, ideologically hostile states in the present international system have managed to find a modus vivendi that will somehow mitigate differences that in the pre-nuclear age would most likely result in war. Many of the subtitles of his chapters are revealing: "The Problem of Loss of Control"; "The Avoidance of Deliberate Violence"; "The Prevention of Inadvertent Violence"; "Limitations on Means, or the Dilution of Coercion." In closing Williams appears to believe that the skill and resourcefulness the superpowers have developed in crisis management may, "if nuclear proliferation continues, . . . become a venerated model for other states to copy, adapt and develop."

I wish that the author had analyzed Brzezinski's proposition in a possible future U.S.-Soviet confrontation: How would a crisis be managed in which the Soviets possess a strategic posture consistent with their war-fighting doctrine and the United States possess a quite different posture associated with MAD (Mutual Assured Destruction)? Under these circumstances would the crisis be managed to the benefit of both sides or to the exclusive detriment of one of the protagonists?

It is a pity that this excellent survey of crisis management literature and superpower crisis practice to date did not project itself into the more dicey context of the future. Only the future will tell if the modalities of crisis management utilized during a condition of general U.S. strategic superiority will change in conditions of strategic superpower equivalency or Soviet superiority. Churchill observed in the late 1940s "if they [the Soviets] behave this way when the stick is green, how will they behave when the stick is dry?" This question needs to be addressed by students of crisis management.

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Contemporary Research in International Relations: A Perspective and a Critical Appraisal. By Dina A. Zinnes. (New York: Free Press, 1976. Pp. xviii + 477. $17.95.)

Nearly 20 years ago, Kenneth Boulding's review of Morton Kaplan's *System and Process in International Politics* described the book as "a giant misstep in the right direction." Zinnes' new book recalls the phrase, as it succeeds in making a major contribution to the field of world politics while falling a good deal short of its awesomely ambitious goals. In a single volume, Zinnes attempts to do no less than: (a) Develop a new methodological taxonomy for world politics; (b) Produce a detailed assessment of the scientific validity of, and the epistemological difficulties attendant upon, each area of research so classified; (c) Trace through these multifaceted conundrums to diagnose a single major impediment to the field's methodological health; (d) Prescribe a cure for the malaise; and, finally, (e) Administer the remedy herself to some of the more promising patients. That she is not always convincingly successful, especially with the last three of these tasks, is scarcely surprising. But the book is so rich in detailed comparative analyses of major works in the field, and so full of valuable mathematical and methodological insight, that readers may wish just to enjoy themselves with these, and ignore their difficulties with its central theme. Unfortunately, the reviewer cannot.

Zinnes contends that approximately 20 years of the widespread scientific study of world politics has resulted, not in the development of a cumulative body of knowledge, but rather in a scattered, often contradictory hodge-podge of unrelated findings. This is due not to the obtuseness of the subject matter, but rather the lack of concern for theory. Only theory can provide true *explanations* of events, fit scattered results into a meaningful pattern, and guide further research toward the most important questions.

Up to this point, one has considerable sympathy with the argument. Too many researchers still appear to regard the theoretical rationale and implications of their work as an uninteresting matter to be dismissed in a few sentences, and do not seem to care where their research is proceeding or why. But Zinnes restricts the term "theory" to formal, mathematical constructs, and relegates all scientific studies *not* employing such models to the category of "ad hoc hypothesis testing." All hypotheses not rigorously deduced from such formal models are "ad hoc," and their empirical test is deemed to make only a marginal contribution to the development of the discipline.

But this is impossibly strict; even some of the natural sciences would be nearly bereft of "theory" so defined. A satisfactory theory must indeed correct known empirical regularities and guide us in the search for new ones (that is, it must explain and predict), but mathematical formalism is only a useful instrument to this end, not an absolute requirement or a goal in itself. Surely Darwin was no less a theorist than Mendelev!

This narrow conception of theory leads Zinnes to take a rather one-sided view of the development of the field. For her, hypothesis testing and data gathering should largely give way to the *construction* and *refinement* of formal models. But this advice is not supported by the early development of the natural sciences. Kepler could not have chosen successfully among several competing models of planetary motion without the extensive and highly accurate observational data provided by Tycho Brahe, and Mendelevian genetics was preceeded by an enormous amount of "ad hoc hypothesis testing" by agronomists and scholars alike. Indeed, in surveying the field one could as easily take the opposite tack, and argue that world politics is still too "data-poor" to allow us to select confidently from a wide variety of equally plausible models. The truth is the theorist and empirist need each other too desperately to permit futile rivalries for pride of place. In fact, they are at present so mutually dependent that they are best combined in each and every world politics scholar.

But if it is bad advice to suggest abandoning data with theory, it is a greater disservice to confuse theory with formalism. The danger is that we may wind up substituting the one for the other, and begin by asking "can we deduce and make deductions from the model?" rather than, "is *it* the right sort of model?" Zinnes' own fascination with the explicit solutions and elegant formal properties of "Richardson-type" arms race models is ample evidence of the dangers in this. Why spend hundreds of pages producing "non-trivial deductions" from a dyadic, continuous, linear, time-reversible model, when most of our empirical evidence *and* theoretical hunches suggest that arms competition in the contemporary international system is an n-adic, discontinuous, nonlinear, time-irreversible process? Our models will always be simplifications, but we have a right to expect them to be correctly specified as far as they go. If they aren't, our endeavors may be "heuristic," but tell us even less about reality than the barefoot empiricist's empty-headed fishing expedition.

Ironically, by choosing different examples, Zinnes could have made a stronger case for mathematical theory than she does. Amazingly, there is no mention of game theory, nor of systems dynamics ("Club-of-Rome") models, both of which have had profound substantive and methodological impact upon the field in recent years. Neither does Zinnes deal with recent efforts by mathematical biologists such as Thom and Zeaman to develop discontinuous, irreversible, and nonlinear models of "catastrophes" such as wars. Such models are often inelegant and mathematically "messy," and for the most part will not yield explicit solutions. They must be explored by approximation, iterative solution, and piecemeal empirical verification. Nevertheless, they are a far steadier base for theory-construction than sterile imports from theoretical physics or economics.

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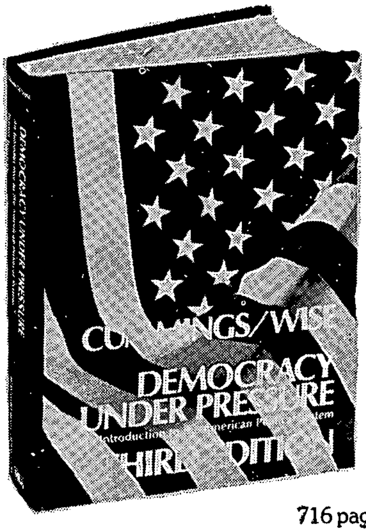

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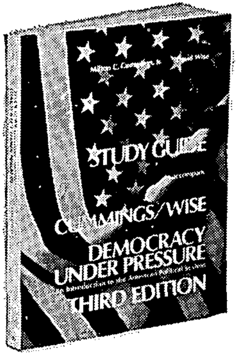

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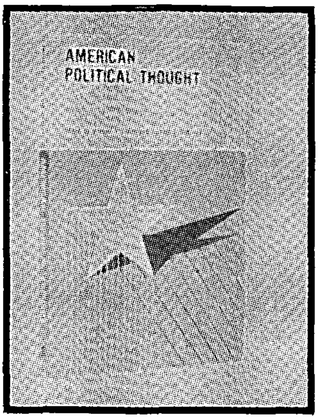

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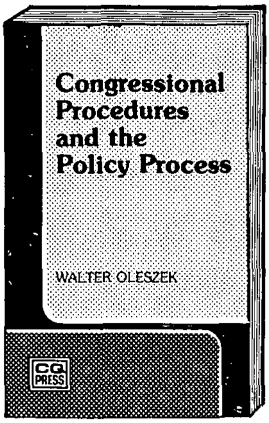
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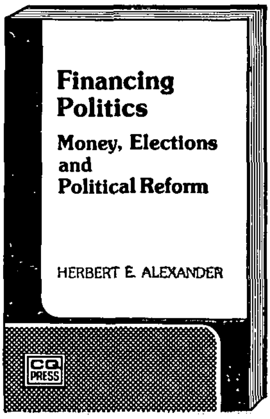
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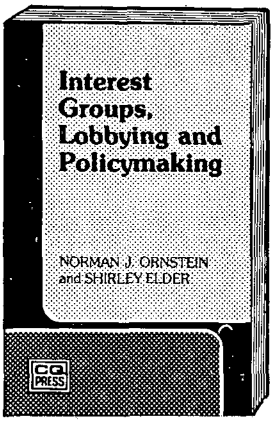
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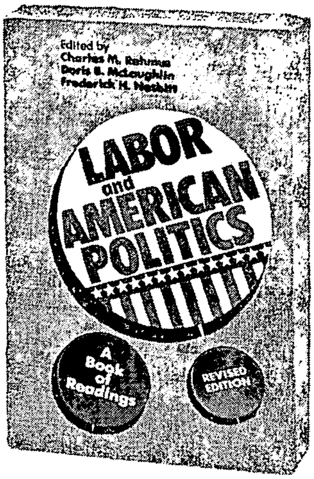
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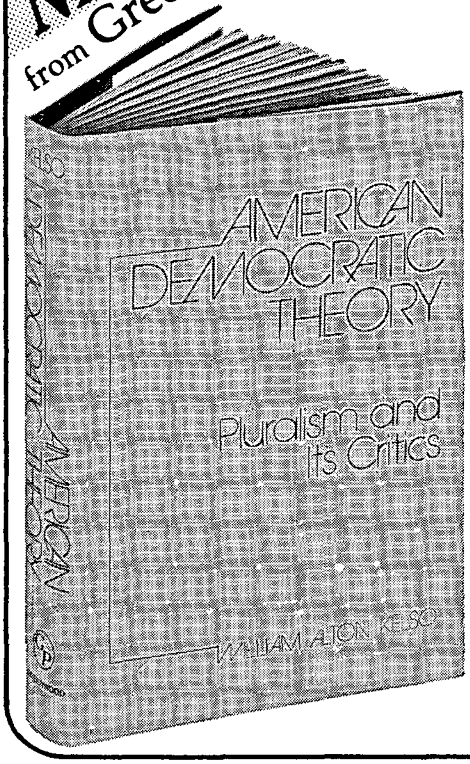
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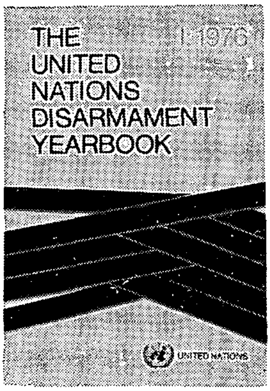

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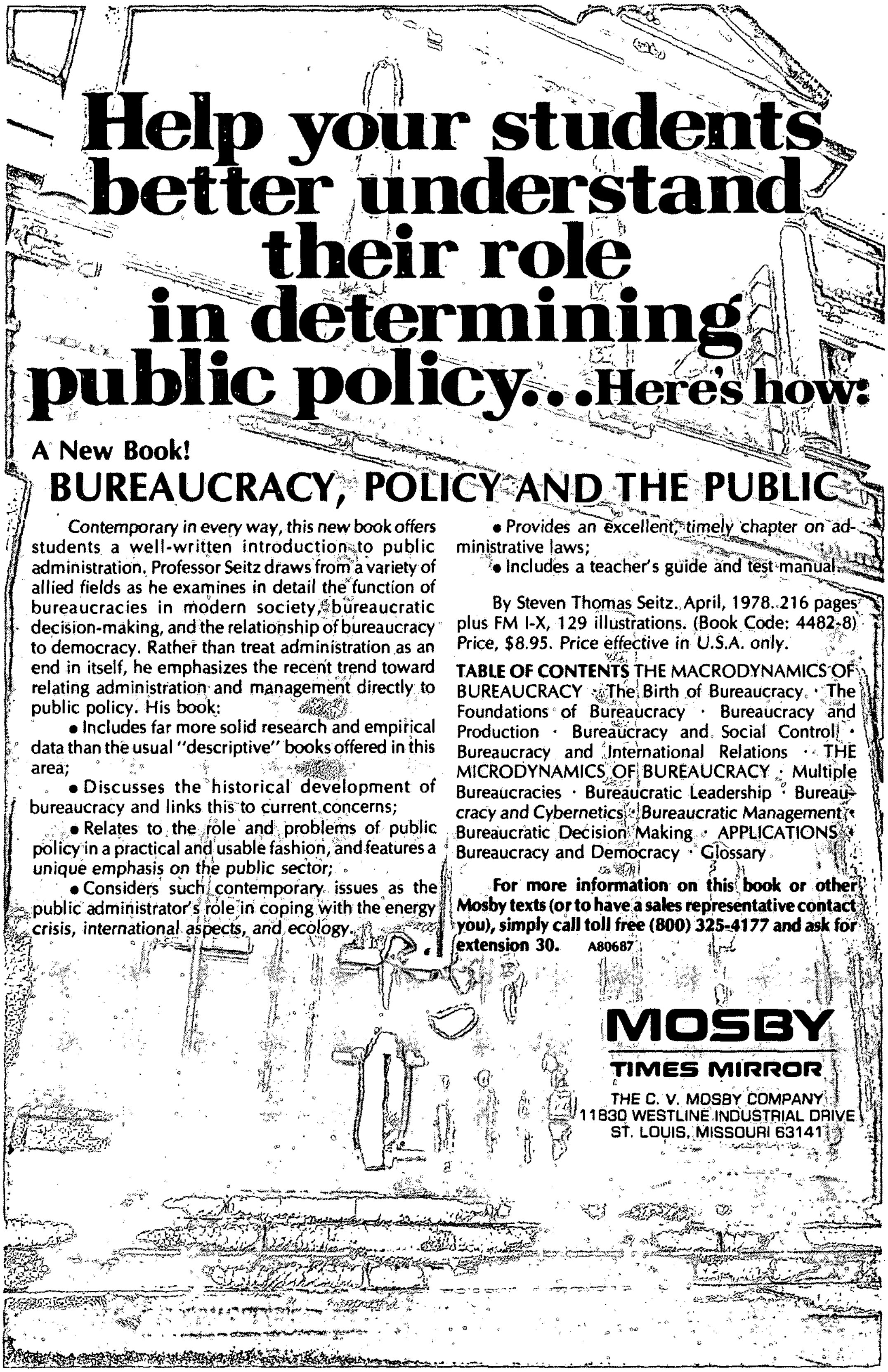

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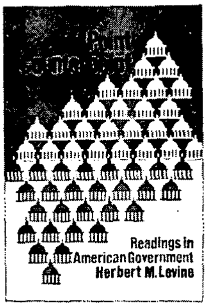

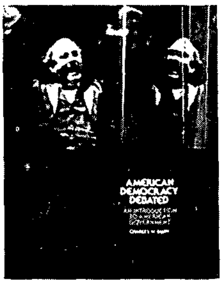

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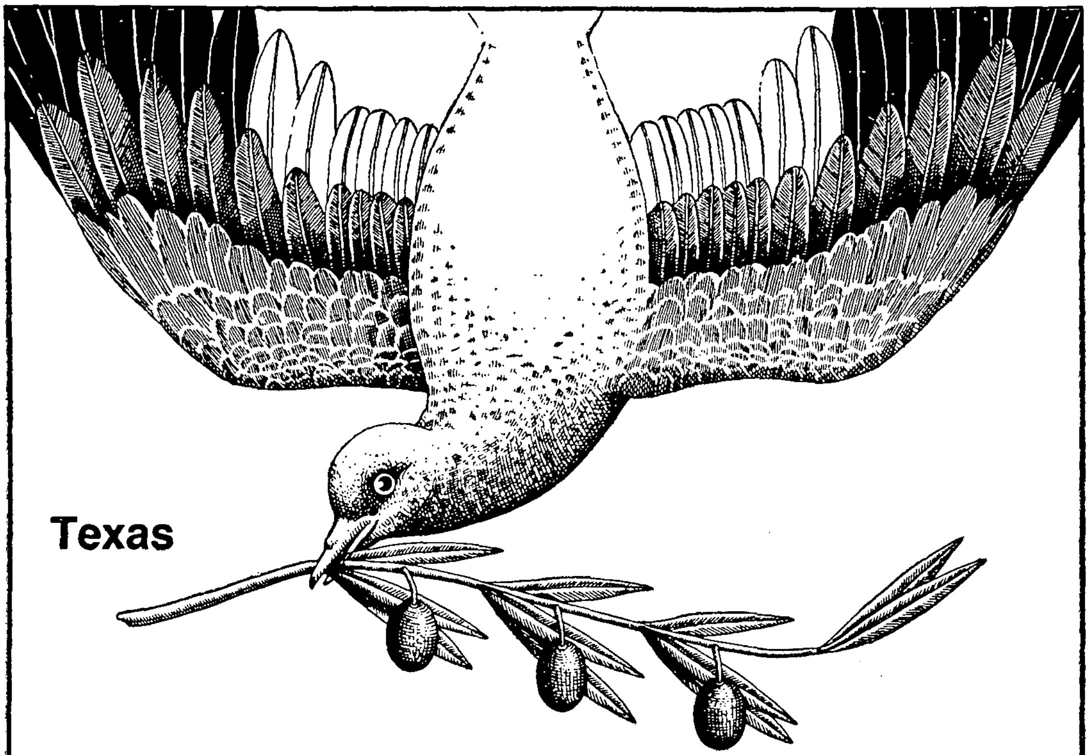

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